

# CRYPTOZOOLOGY

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## A CLASSIFICATORY SYSTEM FOR CRYPTOZOOLOGY

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**ABSTRACT:** Various definitions of cryptozoology are reviewed, and a new classificatory system for the field is proposed. The classificatory system has seven categories. Only Categories II through VI are fully cryptozoological; Categories I and VII do not meet all the qualifying requirements, making them only semi-cryptozoological. The terms "unexpected" and "ethnoknown" are defined, and it is noted that most of the categories in the proposed classification, but not most of the individual claims, concern taxa already scientifically described. The seven categories are objectively defined, and examples of both historical precedents and unresolved claims are reviewed. The classificatory system is intended to reduce subjectivity, increase objectivity, and serve as a tool to future researchers and scholars. It does not dictate what cryptozoology is concerned with, but provides a means through which cryptozoological information can be organized into a logical framework. The classificatory system is flexible, and permits precedents to shift from one category to another in accordance with new facts, or, in the case of claims, with the user's personal preferences.

## INTRODUCTION

The scientific practice of cryptozoology has been of growing interest to scientist and layman alike in recent years. The question of the possible existence of unknown animals—or even surviving "fossil" animals—in our modern age has intrigued many. It has also resulted in much confusion on the part of both proponents and critics of cryptozoology, which, in turn, has stemmed from a lack of a clear understanding of what cryptozoology actually is.

Although most persons interested in the topic claim knowledge of what cryptozoology "is," it is remarkable how different perceptions may be obtained from different individuals. To increase our understanding in this area, the author will review the various definitions which have been proposed, and then propose a new classificatory system which could serve as a practical tool for future researchers and scholars.



## DEFINITIONS OF CRYPTOZOOLOGY

The first clear definition of cryptozoology in the literature is by Bernard Heuvelmans (1974), who described it as "the science of hidden animals . . . in practical terms it means the study of, and the search for, animal species whose existence, because of a lack of specimens or any significant anatomical material, has not yet been officially recognized." This was followed by Roy P. Mackal (1980) who defined it as "an area of study within the main corpus of zoology . . . specifically dealing with the study and search for unidentified living animals." This definition could be misunderstood because "unidentified living animals" would include many millions of living invertebrates, mainly insects, which remain to be described. What makes cryptozoology different is that it investigates certain supportive evidences of the existence of supposed animal species *prior to those species being collected, fully documented, and/or described scientifically*. Such evidences may be 1) indirect, in the form of archaeological relicts, art work, old manuscripts, or oral traditions or legends; or 2) direct, in the form of verbal or written reports of observations of supposedly undescribed animals, from either local or native peoples of an area, or from outside travelers or explorers who visit it.

This prior human knowledge of an animal before its "official" existence is an absolute requirement for cryptozoology, and it is embedded in several more recent definitions given by Heuvelmans, the acknowledged "father" of the field:

The science of *hidden* animals . . . more generally referred to as "unknowns," even though they are typically known to local populations—at least sufficiently so that we often indirectly know of their existence, and certain aspects of their appearance and behavior (Heuvelmans 1982).

A systematized search for unknown species of animals about which some testimonial and circumstantial evidence is available (Heuvelmans 1984a).

The search for animal species still undescribed by science *but on which some information is available* (Heuvelmans 1984b).

The science which deals with hidden animals. "Hidden" animals are animal species or subspecies still unknown to or undescribed by traditional zoology (in fact, unrecognized by a consensus of its representatives) because their asserted or alleged existence is supported either by testimonial and circumstantial evidence only, or by autoptical (i.e., material) evidence considered insufficient by some (Heuvelmans, in preparation).

The scientific study of unknown or undescribed animal forms about which only testimonial and circumstantial evidence is available, or material evidence considered insufficient by some (Heuvelmans, personal communication).

Because of the peculiar station of cryptozoology within the zoological sciences, a precise, all-encompassing definition is difficult to produce. This problem was addressed by the Board of Directors of the International Society

of Cryptozoology at its founding meeting in early 1982. After much discussion, the Board concluded that cryptozoology should be concerned with "animals of unexpected form or size, or unexpected occurrence in time or space." The key work here is "unexpected." So, according to the founders of the Society (including Heuvelmans), to fall within the domain of cryptozoology, an unknown-to-science animal not only has to be known or hinted at prior to its "official" discovery, but it also has to be considered "unexpected" when the official discovery is actually made. "Unexpectedness" is a somewhat subjective term which is not altogether appropriate when attempting to define a concept objectively, and different observers can differ in their opinions concerning *how* unexpected something is. Nevertheless, most would agree that the discovery of a living "fossil" form (as happened with the coelacanth, *Latimeria*) would be quite unexpected in the full and proper meaning of the word.

To incorporate this new qualifier, and to expand the definition of cryptozoology to include all forms of evidence, this author recently proposed the following definition:

The investigation and evaluation of sightings (or other forms of organic, electronic, historical, linguistic, artistic or archaeological evidence) of supposedly unknown, or undescribed, or uncollected, or extinct animals, reported in a consistent fashion by different eyewitnesses (including reports from aboriginal peoples, although the supposed animals may not be unknown to them), and the discovery of which would be considered "unexpected" by zoologists (Greenwell 1985).

Heuvelmans (personal communication) has criticized this definition as being too long and cumbersome, and, in retrospect, his criticism is not without merit. Nevertheless, it is also included here for the record.

## THE PROPOSED CLASSIFICATION

In early 1984, the well-known evolutionist George Gaylord Simpson published a lengthy critique of the concept and practice of cryptozoology (Simpson 1984). While the critique contained numerous interesting points—as well as some surprising errors—it was clear that the author had essentially failed to fully grasp what cryptozoology represented as a scientific pursuit.

In the course of studying his paper, as well as other recent criticisms (May 1984, Diamond 1985), it became increasingly apparent to this author that cryptozoology lacked a descriptive framework within which to organize and analyze its information, and which would reduce the subjectivity which as tended to infiltrate the writings of both critics and proponents. Previous reviews had followed a geographical sequence (unknown animals in South America, Africa, etc.), or a zoological one (mammals, reptiles, etc.). What was lacking was a purely objective classification scheme that, regardless of whether one is a proponent or a critic, could be used to classify crypto-

zoological claims in terms of the kinds of evidence they supposedly represented, regardless also of the geographical or phylogenetic origins thought to be involved.

The classificatory system proposed below evolved over a period of time, benefitting from the review and criticism of various individuals. Seven categories have been erected, only five of which are fully cryptozoological. These are, respectively, Categories II through VI (the reason for this becomes clear below). It is here proposed that, to be fully cryptozoological, categories must necessarily have 1) at least one historical precedent, demonstrating that such a claim, however unlikely, proved to be correct in the past, and 2) a current unresolved claim.

The seven categories are objectively defined below:

*Category I:* Individual representatives of known, extant species whose form or size is unusual or unique for the species, or whose coloration or pattern is in some way extraordinary (because only individual—not species—variation is involved, and relative rather than absolute differences are dealt with, this category is not cryptozoological in the complete sense of the definition).

*Category II:* Extant and well-known taxa unrecognized as existing in certain geographical areas because of inconclusive evidence.

*Category III:* Presumably extant taxa, not representing fossil forms, described from only limited organic evidence, such as skin, bone, or tissue, with no complete specimens ever collected.

*Category IV:* Known taxa which supposedly became extinct during historical times, but which may have survived far longer than originally believed, or may have survived to the present.

*Category V:* Representatives of fossil forms believed to have become extinct during geological times, but which may have survived into historical times, or even to the present.

*Category VI:* New taxa of known, extant forms, for which no known organic evidence exists.

*Category VII:* New taxa which, although “unexpected,” were not known previously to aboriginal peoples or other eyewitnesses, although they may have fossil representatives (because of the presumed lack of human knowledge prior to the discovery of these animals by science, this category is not cryptozoological in the complete sense of the definition).

A new term, “ethnoknown,” is also proposed, in order to simplify and reduce a concept into a single word. “Ethnoknown” is used to represent prior human knowledge of an animal, as discussed above, whether it be in the form of modern sighting reports, ancient depictions or manuscripts, or oral traditions or legends. Such prior human knowledge may come from

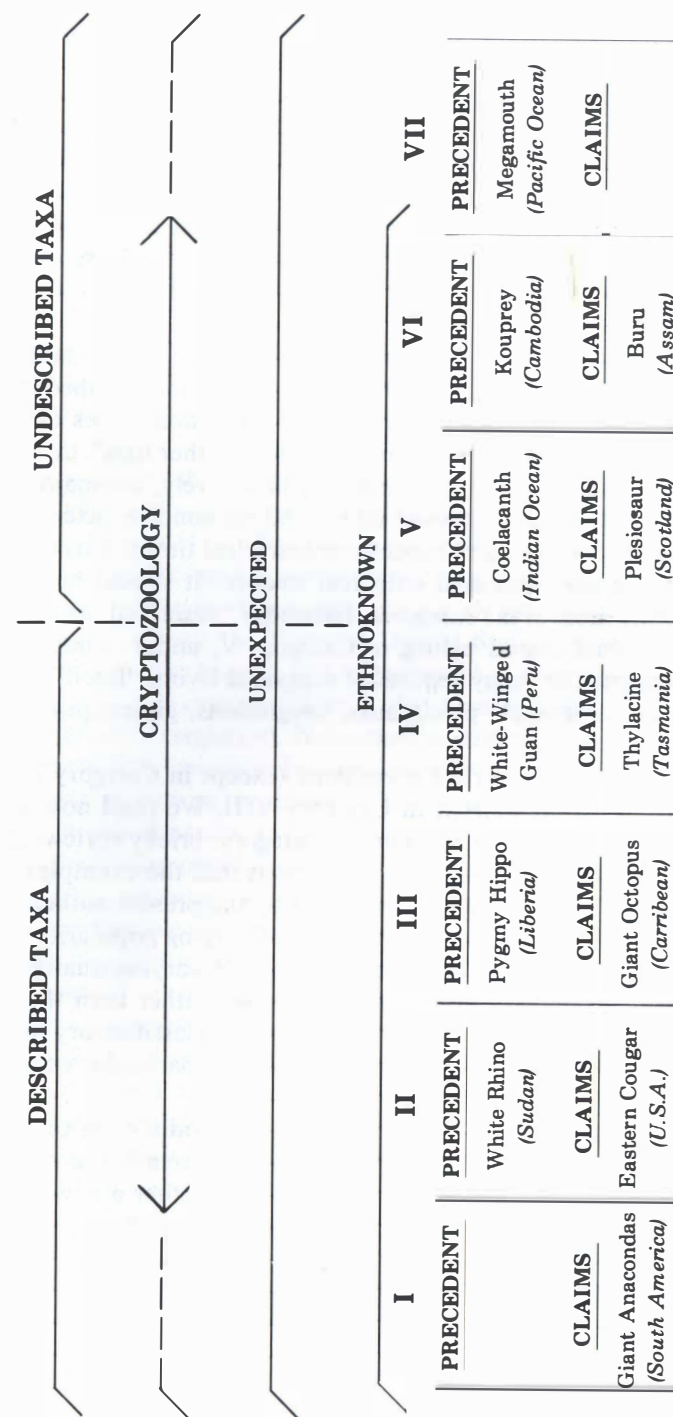


FIG. 1.—A schematic representation of the proposed classificatory system for cryptozoology. Although all seven categories fall under the concept of “unexpectedness,” Categories I and VII are considered only semi-cryptozoological. Four of the categories concern taxa already scientifically described as existing in historical times. The examples given under Claims are not necessarily supported by the author.



both native peoples (Scots living alongside Loch Ness are here considered "native peoples"), as well as traveling outsiders or explorers. The term "ethnoknown," then, simply means *some* kind of human knowledge prior to full scientific discovery and description, and this requirement is already a basic tenet of cryptozoology (Heuvelmans 1982, 1984a, 1984b).

In Fig. 1, the seven categories are spread out in schematic form. It will be observed that, while all fall under the concept of "unexpectedness," as discussed previously, Categories I and VII are considered only semi-cryptozoological. In addition, Category VII does not fall under the important concept of "ethnoknown."

A final observation is that four of the seven categories come under the main heading of "described taxa," and only three under "undescribed taxa." On the one hand, this represents a problem for those definitions of cryptozoology which address "unknown" animals. On the other hand, this demonstrates that, when cryptozoology is classified objectively, the majority of its categories (at least in this proposed classification) concern taxa already scientifically described as living in modern or historical times, thus refuting some notions that it does not deal with real science. It should be added, however, that, while most of the *categories* fall under "described taxa," most of the actual individual *claims* belong to Category V, under "undescribed taxa," which concerns the many reports of supposed living "fossil" forms, such as pterosaurs, dinosaurs, plesiosaurs, zeuglodon, gigantopithecines, and neanderthaloids.

Fig. 1 lists examples of historical precedents (except in Category I), and examples of current claims (except in Category VII). We shall now move through the classification system, category by category, briefly reviewing the examples given in Fig. 1. It should be emphasized that the examples used for current claims are not necessarily supported by the present author; they merely are used as examples to represent claims of varying popularity made by others at different times, and based on evidence of varying quality.

Some readers may prefer to insert other examples rather than the ones given, and that is to be expected. It is the proposed classificatory system itself which the author thinks is relevant here, not the particular examples used.

**Category I Precedent:** There is no clear precedent available in this category, simply because the evidence being claimed is of a relative rather than an absolute nature. It is not a clear-cut question of whether a new species exists or not; rather, it concerns to what degree an individual of a scientifically *known* species differs from the norm in terms of size, coloration or pattern. For these reasons, this category is not considered fully cryptozoological, and the corresponding area is left blank in Fig. 1.

**Category I Claim:** The example used in Fig. 1 concerns giant individuals

of the South American anaconda, *Eunectes murinus*. Most herpetologists believe that 30 feet is about the length limit for the anaconda, as well as for the Afro-Asian pythons. There are many reports of larger snakes, and a \$50,000 reward still stands with the New York Zoological Society for a live snake measuring over 30 feet. Reports of giant estuarine crocodiles, giant white sharks, and giant sturgeon also fall under this category. Many of these reports probably result from exaggerated stories, but some may involve actual individual giants (Wood 1982). It should be emphasized here that this category deals only with *known* species. If it later turns out that a "giant" anaconda or a "giant" white shark belongs to a new, previously undescribed species—or even a fossil species—then this claim (which would then actually become a precedent) would shift to a different category.

**Category II Precedent:** The white rhinoceros (*Ceratotherium simum*) was originally known to zoology only from southern Africa, but it was subsequently discovered as a subspecies (*C. s. cottoni*) in Sudan, a distance of about 2,000 miles (Lydekker 1901). This late discovery of such a large terrestrial mammal in what was thought to be a well-explored, open-range area, has implications for other areas of cryptozoology.

**Category II Claim:** The puma, cougar, or mountain lion (*Felis concolor*) is found throughout the Western United States, as well as most parts of Latin America. The Eastern U.S. subspecies has been considered extinct, however, for almost a century (there is also a precariously surviving subspecies in the Florida Everglades). Nevertheless, sighting reports from throughout the Eastern U.S. continue to come in (Downing 1984), despite the lack of federal recognition or protection. If it is eventually established that breeding cougar populations do, in fact, exist in the East, it will be necessary to determine if these populations represent the original subspecies now thought extinct (*F. c. cougar*), migrations of other subspecies to the Eastern U.S., or released captive animals representing any number of subspecies—or even all three possibilities occurring in unison.

**Category III Precedent:** According to Willy Ley (1948), the pygmy hippopotamus (*Choeropus liberiensis*) was described by Samuel G. Morton in the 1840's based on a single skull sent to him from Liberia (Morton 1844). As it later turned out, this animal was quite different anatomically from the hippopotamus, and was not just a pygmy version of it. Specimens were sent to Holland and Germany later in the 19th Century, but it took Carl Hagenbeck, the famous German animal collector, to bring out a live specimen, in 1913, by which time disbelief in the animal had set in. Hagenbeck sent Hans Schomburgk to Liberia to do the job. Heuvelmans (1958), however, states that a young pygmy hippo was acquired by the Dublin Zoo in 1870, but that it died soon after arrival. They concur, nevertheless, that Schomburgk was probably the first white man to see the pygmy hippo in its natural environment, for whatever distinction such a feat may bestow. Although



some of the events surrounding the "discovery" and description of the pygmy hippo are unclear, it is clear that the original scientific description by Morton was made long before a complete specimen was at hand, and, in the interval, doubts arose as to the very existence of the animal.

*Category III Claim:* The giant octopus was originally described by A. E. Verrill in 1897, based on an organic mass which had washed ashore at St. Augustine, Florida (Verrill 1897a). Although Verrill subsequently rejected the octopus hypothesis (Verrill 1897b), the scientific name, *Octopus giganteus*, still applies—but only (if the author correctly understands the International Code of Zoological Nomenclature) when and if the St. Augustine material turns out to be, in fact, from a giant octopus, irrespective of whether other giant octopuses are found to exist. The case was resuscitated in the 1960's, and modern scientific analysis of the St. Augustine material—which had fortunately been preserved at the U.S. National Museum—hinted at octopus tissue rather than squid, whale blubber, or other marine organic matter (Wood and Gennaro 1971). Since then, amino acid analysis has further supported the octopus hypothesis (Gennaro and Mackal 1985), and further work involving immunological analysis is to be carried out shortly (Mackal, personal communication).

*Category IV Precedent:* The white-winged guan (*Penelope albipennis*) is a relatively large Peruvian bird of the family Cracidae believed extinct for a century, until the living species was rediscovered in 1977 by an ornithological team from Louisiana State University (de Macedo-Ruiz 1979). By their nature, birds are very susceptible to species depletion and extinction, and, of all vertebrates, they are the least represented in the annual finds of new species (Heuvelmans 1983). It was thus significant that a bird of such large size, weighing up to 4.5 lb. and measuring over 3 feet, persisted so long in an "extinct" state, particularly as it was actively sought between 1952 and 1968. It subsequently was learned that the local inhabitants knew the bird well in its northwestern Peru habitat.

*Category IV Claim:* The thylacine or "Tasmanian tiger" (*Thylacinus cynocephalus*) was last known in a wild state in 1930, although the last captive specimen lived until 1933 in the Hobart Zoo. Since that time, innumerable eyewitness sightings and footprint finds lend support to the notion that it may yet survive in remote forest areas of Tasmania (Rounsevell and Smith 1982). Nevertheless, expedition after expedition, right up to the present time, has failed to produce a specimen, or even substantial evidence of the species' survival (Guiler 1966, Smith 1982). Its eventual rediscovery, should it ever occur, would demonstrate that even a known animal can avoid definitive scientific detection over at least a 50-year period. (Reports of thylacines on mainland Australia also occur from time to time. However, as the species supposedly became extinct there in the late Pleistocene—rather than the 20th Century—such claims properly belong to Category V).

*Category V Precedent:* The coelacanth fish, *Latimeria*, represented a form believed extinct since the end of the Cretaceous. However, it was discovered alive in late 1938, and a complete specimen, after many years of intensive searching, was found in late 1952 (Smith 1957). Despite the living coelacanth's significance to paleontology and ichthyology, Comoro Islanders had long used coelacanth scales to wipe the mud off of their bicycle tires. Although there have been a number of other "fossil" animals found in historical times, even to the present, the coelacanth is usually considered the most important and striking example, and it is usually cited by those supporting modern claims of "fossil" animals. (As noted above, in the definition of Category V, we are dealing with "representatives of fossil forms," rather than the "fossil animals" themselves; there are usually some anatomical and physiological distinctions between the representatives of fossil forms, when discovered, and the original fossil animals, which has even been used as an argument in an attempt to reduce the significance of such finds [Simpson 1984:30].)

*Category V Claim:* Plesiosaurs, like their Mesozoic counterparts the dinosaurs and pterosaurs, supposedly became extinct at the Cretaceous-Tertiary boundary, or soon thereafter. The sightings of supposed large animals in Loch Ness, Scotland, which first drew worldwide attention in the 1930's, led a number of persons to propose that, because of morphological features described in eyewitness accounts, plesiosaurs had survived to our own time (Dinsdale 1961, Meredith 1977). However, Nessie, as "the monster" became known, has the distinction of having had more genera, orders and even phyla assigned to it than any other supposed animal in the annals of cryptozoology. Besides plesiosaurs, zeuglodon (also thought extinct since geological times), large pinnipeds, killer whales, giant amphibians, giant eels, and even giant invertebrates have been proposed over the years, all based on how the respective authors interpreted the evidence. Thus, although the plesiosaur hypothesis has been the most visible in the popular media, serious consideration has been—and should be—given to alternative hypotheses.

As stated above, Category V represents the area of cryptozoology most discussed by both proponents and critics, sometimes to the exclusion of all others. While it is certainly a most interesting and potentially important area, this emphasis, in this author's opinion, has resulted in some of the misconceptions as to what cryptozoology actually is. However, because of the emphasis which does exist, correctly or incorrectly, we may briefly review other claims in this category.

The survival of Mesozoic sauropods has been proposed to account for Mokele-Mbembe, a bulky animal reported in the swamps of Central Africa; the present survival of pterosaurs in Africa has also been proposed. The present or recent survival of Pleistocene mammals, such as mammoths in Siberia and ground sloths in South America, has also been suggested. Finally,



sighting reports of "wild men"—from most parts of the world—have been attributed to fossil hominoids or hominids, again depending on how the authorities involved interpret the evidence. The most prevalent hypothesis for the North American Sasquatch and the Chinese Wildman is that they are living representatives of the fossil genus *Gigantopithecus*, while the Soviet-Mongolian Almas is thought by many to represent living forms of *Homo*, of the Neanderthal lineage. Further extensive discussions of all these supposed instances of persistence from geological times to the present may be found in the works of Green (1978), Heuvelmans (1958, 1978, 1980), Heuvelmans and Porshnev (1974), Mackal (1980), Markotic and Krantz (1984), Sanderson (1961), and Shackley (1983). Heuvelmans is also currently preparing a comprehensive checklist which will include all categories of cryptozoological reports worldwide (Heuvelmans, personal communication).

*Category VI Precedent:* The kouprey (*Bos sauveli*) is a Southeast Asian wild ox discovered in Cambodia in the 1930's (Urbain 1937, Lekagul 1952). It was totally new to zoology, and it ranks as one of the largest terrestrial mammals found in modern times. The kouprey was thought to have become extinct in recent decades, but living specimens were found in the early 1980's, despite intense hostilities among the human populations of the area. Its future is precarious indeed.

*Category VI Claim:* The Buru was a large lizard-like animal which supposedly inhabited a valley in Assam, in northeast India (Izzard 1951, Mackal 1980). Although the natives of the area stated that all the reptiles had been exterminated, it is reasonable to assume that they were large monitor lizards (*Varanus*), as are the Komodo dragons. There have been repeated reports and rumors of giant monitor lizards, as large or larger than the Komodo dragon, from various parts of Southeast Asia, New Guinea, and Australia, and the question of the Buru, in this author's opinion, must be evaluated in this context. Certainly, giant monitor lizards 15 or even 20 feet in length would represent totally new species for zoology.

*Category VII Precedent:* The example used in Fig. 1 is megamouth (*Megachasma pelagios*), a new shark discovered in 1976 off Hawaii, and described scientifically more recently (Taylor, Compagno, and Struhsaker 1983). Also recently, in late 1984, a second specimen was recovered off the coast of Los Angeles, California. Megamouth is not only large (15 feet) and unusual as a filter-feeding shark, but it represents a new species, a new genus, and, according to Taylor et al., a whole new family. The finding of such new taxa, even in the oceans, is a relatively rare and "unexpected" event. However, as seen in Fig. 1, this category does not fall under the concept of "ethno-known," and it is thus reduced to a semi-cryptozoological status. Heuvelmans has previously stated that such instances are not cryptozoological at all, but merely illustrate what sorts of animals might be found if cryptozoology were pursued diligently (*ISC Newsletter* 1984).

*Category VII Claim:* Because there is no human knowledge of these animals prior to scientific discovery, there can be no claims, which negates this category from being fully cryptozoological. Thus the corresponding area is left blank in Fig. 1.

## DISCUSSION

It is proposed that the above classificatory system could serve as a useful tool to future researchers and scholars when discussing the content, the successes, and the failures of cryptozoology. By restricting discussions to one category only, or to one category at a time, it may be possible to stimulate more objective analyses, and less subjective and emotional responses by participants in such discussions.

It should be noted that the classification does not attempt to dictate what cryptozoology is, or should be. It merely reflects what cryptozoology has been, based on an extensive review of the literature. It has taken the existing information in the literature and reassembled it to create a logical framework for future reference.

It should also be noted that, although the classification itself remains static, it provides great flexibility to the user. For example, the user may list any number of precedents under each category as he or she may locate in the literature; the individual examples given in Fig. 1 can surely be expanded upon. Furthermore, individual examples have the flexibility of shifting from one category to another, depending on what new information may become available. Thus, the kouprey belongs in Category VI with reference to its initial discovery (in the 1930's), but shifts to Category IV with reference to its rediscovery (in the 1980's). Likewise, megamouth remains in Category VII, banished forever from full cryptozoological status; but maybe not: if it were discovered in the future that some Pacific islanders had been acquainted with megamouth all along—even though we were unaware of their prior knowledge—megamouth would immediately shift to another category—in this case to VI.

Similarly, the user may list any number of claimed animals under each category as he or she may locate in the literature, and here there is more flexibility still, as the user is not bound, in this case, by the realities of historical fact, and he or she may place those claims found most appealing or palatable under the desired categories. Furthermore, if a change of mind—or heart—should ever occur concerning the zoological affinities of a reported animal, even shifting it from reptile to mammal, the classification permits this to be reflected accordingly; a supposed animal such as Nessie can shift from Category V to Category VI, if, for example, one prefers to reject the plesiosaur hypothesis and to postulate that a modern but unknown taxon, such as a large pinniped, is involved.

This classificatory system has been designed to facilitate theory and prac-



tice in cryptozoology, and has benefited from the thought and criticism of numerous individuals. The author invites further constructive criticism from interested parties.

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## ON THE POSSIBLE IDENTIFICATION OF THE EGYPTIAN ANIMAL-GOD SET

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**ABSTRACT:** The ancient Egyptian god Set was pictured with the head of an animal, as was true of many Egyptian deities. This animal, however, unlike the others, has never been identified. Examination of pictorial representations, as well as behavioral characteristics gleaned from ancient texts, affords a consistent model of the animal involved. An unclassified canid, possibly still in existence and related to some of the "Nandi Bear" tales, is indicated.

### INTRODUCTION

The purpose of this article is to review the beliefs of the ancient Egyptians, and those of modern Egyptologists, about the animal, if any, attributed to the major god Set or Sutekh. The potential candidates are evaluated using a variety of ancient sources, modern zoology, and published cryptozoological material. A possible unclassified species is one of the most likely conclusions to this long-standing problem.

Dynastic Egypt had a religion filled with many intriguing deities, most of which had some animal characteristics, and were pictured with animal heads. A few of the most popular are illustrated here as Fig. 1.

The god Set had a complex and seemingly contradictory role in Egyptian religion. Generally, he was pictured as powerful and dangerous, and often simply evil. He killed Osiris, dismembered him, fought Horus, and tore out his eye, and he threatened to do likewise to Pharaoh and the everyday citizen. He is a leading candidate as a prototype of the Judaeo-Christian Devil (Russell 1977), and the name "Satan" may well derive originally from Set.

As the opponent of life and light, and the personification of evil, Set became bound to the evil Titan Typhon in Alexandrian times. Satan's medieval Christian form also owed much to Pan, as well as to Set-Typhon. But this was the view of Set in the eyes of worshippers of Osiris, and there is ample evidence that Set was revered as a powerful protective local god in Upper Egypt long before the coming of Osirian religion, and the consequent relegation of Set to his solely demonic role.

It will not be necessary to attempt to unravel the history and development of the Set concept in this article. Interested readers are referred to the books *God of Confusion* (TeVelde 1967), *The Origins of Osiris* (Griffiths 1966), and *The Conflict of Horus and Seth* (Griffiths 1960) for more details. In summary, the following list of ideas expresses my own conclusions upon this poorly understood and widely debated subject:





FIG. 1.—The Hawk-headed Sun-god, the Jackal-headed god of mummification, the Ibis-headed god of “science” and magic.

- 1) Set was originally a local god in Upper (Southern) Egypt, possibly as early as the predynastic Nagada culture.
- 2) Like most, if not all, Upper Egyptian deities, Set was zoomorphic (animal-formed), in contrast to the largely anthropomorphic gods of the Nile Delta.
- 3) Set's followers represented a power in regional politics in Upper Egypt, perhaps *the* power. Unification of Egypt under the kingship of Horus-devotees may have been symbolized in the Horus-Set conflict.
- 4) Later, upon arrival of Osirian religion (in its ascendancy in the 5th Dynasty), myths became bent and invented anew to accommodate the new power. Set became the epitome of evil, and acquired certain “alien” characteristics.
- 5) Set continued to maintain a strong following even then, and is equated often with the power of the South. The other theriomorphic gods of certain Upper Egypt cities (partners in the Southern alliance) are thereby given “Sethian” qualities.

These changes in the religious and political roles of Set allow us to eliminate later contaminations of the god's nature, and to clarify our picture of the animal originally involved.


#### THE FORM OF SET

Representations of Set in pre-dynastic Egypt are rare and debated. The most convincing is the mace-head of King Scorpion in the “protohistoric” period (ca. 3000 B.C.) (Fig. 2). Egyptologists are fairly certain that this form is meant to represent Set, since it contains the four most visual characteristics: 1) a canid-like quadrupedality; 2) an up-raised tail; 3) a long, hooked muzzle; and 4) peculiarly squared prick ears.

Once writing flourishes in the Pyramid texts, Set animals appear volu-



FIG. 2.—The mace-head Set figure (ca. 3000 B.C.).

minously in the hieroglyphics of the 5th Dynasty Unas Pyramid. This simple stick-form glyph is written thus: . Later hieroglyphs for Set became more elaborate (Fig. 3). The last of these is a typical later Osirian “contamination”: Set as the ass which threshes the Osirian wheat, with a knife in his snout. Two other examples of Set-forms from Egyptian art appear in Fig. 4.

#### EGYPTOLOGICAL CANDIDATES

There seem to have been as many animals related to Set as there were animals to choose from. Egyptologists have been hampered in their work, not only by the unusual physical characteristics of the drawings, but also by the variety of animals spoken of as “Sethian” in the old texts. Here is a certainly incomplete list of proposed candidates: crocodile, hippopotamus, serpent, ass, camel, fennec, jerboa, hare, long-snouted mouse, pig, oryx, giraffe, gazelle, auroch, hyena, European boar, orycteropus (aardvark), long-snouted mormyr, various canids, and even a bird (TeVelde 1967).

In the face of all this confusion, three possibilities arise:

- 1) the Set animal, whatever it was, is now gone, probably hunted to extinction by the Egyptians themselves;
- 2) the Set animal never existed; it was a fabulous creature meant to represent the awesome power of darkness and death;
- 3) the problem is so complex it is insoluble, so we might as well not try to solve it.



FIG. 3.—Later hieroglyphs representing the animal-god Set.



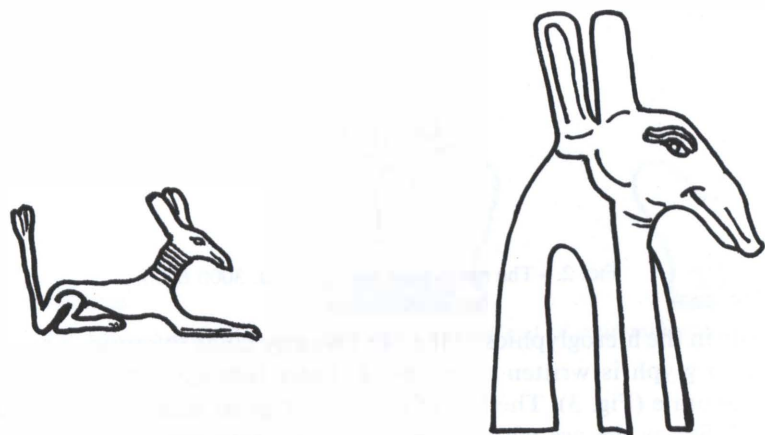


FIG. 4. — An Old Kingdom drawing of Set reclining (TeVelde 1967), and Set in typical wiggled-god form.

To any legitimate researcher, possibility 3) deserves little respect; this author believes that possibility 2) is not viable in this case either. The reasons are simple, even obvious. *All* other theriomorphic deities of Set's antiquity have been clearly associated with known animals. And all of these "contemporaries" of Set display coherent behavioral characteristics of such animals in their myths, a display apparently also true of Set. Most Egyptologists seem to feel that, despite the difficulty of pinpointing what Set was, a real animal was behind it. An expression of this position from one of the most famous scholars in the field follows:

In the absence of any facts on the subject we must assume that the animal which was the symbol of Set was one that prowled about by night in the deserts and in the waste places of the towns and cities, and that his disposition was hostile to man, and wicked generally, and that owing to his evil reputation he was hunted and slain with such diligence that he became extinct in comparatively early times [Budge 1969].

We can eliminate the majority of the choices in the above list rather quickly, thus reducing our analysis to manageable proportions. These eliminations fall into three categories:

- 1) There are many "Sethian" animals which are obviously not *the* Set animal, but which represent other theriomorphic gods and goddesses related to Southern Egypt. These include the gazelle, the pig, the crocodile, the hippopotamus, the mormyr, the oryx, the aurochs, and even the turtle. Note that all are real animals (the mormyr was a sacred fish), although one, the aurochs, is extinct.

- 2) There are two further animals which became associated with Set later, due to alterations caused by Osirian religion: the serpent and the ass. No one has mistaken snakes for Set, but a word on the ass-onager is required. This animal was once associated with the sun-god, but got mixed into the Set story by a series of convolutions involving a myth where Osiris sits on Set (to show dominance), then rides on him (as a *boat* across the sky), then is dismembered by him (as Osirian grain is threshed in ass-drawn mills), and finally rides him as an ass. Thus are myths and muddlements made.
- 3) Several other animals have been proposed, apparently out of desperation and without any regard to the content of the ancient texts, simply in the hope of at least partially matching the appearances. These include the European boar, the camel, the long-snouted mouse, the fennec fox, the hare, the giraffe, the jerboa, and the armadillo. None of these relate at all well to the animal behaviors of Set, and none match the physical appearance well enough to rationalize ignoring that important inconsistency. Concerning the armadillo, however, there is a slight chance that it may play a role. Its abandoned burrows are often used by animals which very much resemble the Set animal in its behaviors.

Thus, we are left with the following possibilities: a) known canids; b) crypto-canids; c) hyenas; d) crypto-hyenas; and e) an as-yet unclassified animal form.

#### THE CHARACTERISTICS OF SET

The following details constitute the significant behavioral and physical attributes of Set, as extracted from ancient texts: 1) strength, aggressiveness, and viciousness; 2) a prowler and stalker; 3) a wild beast of the night; 4) a carnivore and a carrion-eater; 5) a stealer and dismemberer of dead bodies; 6) an odor-tracker; 7) foul smelling; 8) maker of a loud noise; 9) a spewer of dangerous saliva; 10) having reddish or reddish-brown color and possibly a black face (Budge 1969); 11) one similar to the "Sebau fiends" of the *Book of the Dead* (Budge 1909), who come to kill and dismember in packs; 12) a canid-like form, larger than a jackal; 13) an erect, tufted tail even when seated; 14) a curved snout held near ground when tracking; and 15) long pricked squared or flattened ears.

Some sample texts describing these qualities of the Set animal are quoted below:

Deliver me from this god who seizes souls and licks that which is rotten, who lives on offal and is in darkness and obscurity, who terrifies the weary—it is Set! [TeVelde 1967].

The great Ennead has saved thee . . . They have prevented him from spluttering his saliva against thee [Griffiths 1966].



Get back, Blackface, skilled with your smell, dweller in the Mansion of the Desert, disturber who is sent in storm. O you who live by your thefts . . . (the deceased) will not give his powers to the messengers of Set who live by their thefts [Faulkner 1977].

Purify this place of the confederates of the Malefactor (Set), who have come from the slaughterhouses and whose smell is still fresh [Clark 1959].

There are two other sources which may add some insight into this creature. In the hieroglyphs, Set's name was transliterated *štš*. Using this as a stem-word, two other characteristics often applied to Set were written as *tštš* (to cut to pieces) and *tši* (to turn away or desert). The point of this is that the Bible speaks of a creature whose Hebrew name transliterates to *tsiy*. It appears at least six times in Psalms, Isaiah, and Jeremiah (United Bible Societies 1972). It related to a wild desert animal which howls or yelps, and which was envisioned sometimes as a hyena, a cat, a demon, a night monster, or as the "people" inhabiting the desert (the ravenous beasts which devour human bodies).

Another possibly related point from the ancients: the old peoples tended to make a distinction between dogs and hounds (Merlen 1971). Hounds, although still dangerous, were human bred and "good." Dogs, obviously dangerous, were wild and "evil." The idea of the dog and the Hound of Hell as the embodiment of an ultimate evil power has come down through all history to us today. The wolf often bears the brunt of this stigma, much to its detriment. The Bible portrays dogs as wild destroyers and dismemberers.

A more modern set of myths may also relate to our story. Some anthropologists believe that the religion of the Yoruba people of West Africa consists partially of ideas dating back to Egyptian concepts. Their evil deity, Esu, supposedly incorporates "Sethian" characteristics (Lucas 1970). Esu seems to be like a "savage dog." The god rules the nocturnal hours, and has dogs sacrificed to him. Esu's other name, Obalufon, is derived from Typhon, the Greco-Roman name for Set.

A further cluster of African ideas involves Eastern and Central African legends of the "*kerit*" or "*chimiset*." The latter name is intriguing in that it incorporates Set's name. Some of the attributes embedded in the tales of this animal are "a howling, hideous cry," "nocturnal," "something like a hyena," "large and savage," "reddish or black fur," "plume tail," "very long standing ears" (Hichens 1937, Heuvelmans 1958). Heuvelmans reports that the *chimiset* is felt to be the "devil," and that the urine "is said to be so evil smelling that no man can stay near it." The collected tales have been grouped together in an attempt to illuminate a possible cryptozoological species named "the Nandi Bear" by white settlers. The tales seem to be telling more than one story, however, at least to this author. Some, as in the details mentioned above, sound quite like the Set animal; others, are not at all analogous. One may well wonder if the Set animal, or something like it, is a *part* of the Nandi Bear mystery.

## CANIDS AND HYENAS

There are eleven African representatives of the wild Canidae known to exist today. There are six foxes, three jackals, the Abyssinian wolf, and the wild hunting dog. Five of these forms (four foxes and the golden jackal) are still native to the Egyptian region. The wild dog, *Lycaon pictus*, has a northernly range just approaching the southern boundary of modern Egypt (Fox 1975).

The foxes seem to have few attributes relating to Set, but do point out something worth keeping in mind. When speaking of the sand fox, *Vulpes pallida*, a prominent text states: "Almost nothing is known about this species" (Fox 1975). When speaking of Ruppell's fox, *Vulpes ruppelli*, the same text repeats: "Not much is known about this species which inhabits the stony desert in Northern Africa." The point, which will be obvious to cryptozoologists, is simply that the Sahara desert region continues to hold many zoological mysteries, and probably a number of uncatalogued species as well.

The golden jackal bears much more resemblance to Set. It is the most nocturnal of the jackals. It is mostly carnivorous, and a scavenger. It has pale reddish-brown fur, and hunts in groups (although not usually organized ones). But the jackal's diminutive size is against it. The Bible referred to the jackals as "the little foxes," a far cry from the demonic *tsiy*. The Egyptians saw the jackals as much less fearsome than Set, and had several small dog/jackal deities, including possibly the original form of Osiris himself.

This leaves us with *Lycaon pictus*, the wild hunting dog. This animal has been placed in a separate sub-family from the wolves and dogs in the genus *Canis*, largely on anatomical grounds (i.e., only four digits on each foot) (Fox 1975). Molecular biology and chromosome counts, however, indicate that they and several other canids are probably interfertile (Fox 1975). *Lycaon* has the same number of chromosomes (78) as several other common canids, such as *Canis aureus*, *Canis familiaris*, and *Canis lyas*. This is often taken by molecular biologists as indicating genetic closeness. *Lycaon pictus* has many Set characteristics, including a very powerful, musky odor, and the "blackface," but fails in several important areas: non-nocturnalness, distinctive non-ruddy coat, and the facial structure dissimilarities. A side-note: when the *Lycaon* bitch is raising pups, she inhabits the burrows of the aardvark. Jackals do something similar. A minor theory for Set, therefore, could involve a synthesis of characteristics between the aardvark and a wild canid.

There is, of course, another canid of Egypt which is quite like Set, the ancient hunting greyhound-like variety of *Canis familiaris*. In order to evaluate whether a Set-like animal could be a relative of the ancient greyhound, it would be valuable to know where these hounds came from. Unfortunately, theories on the origins and evolution of dog breeds are in even more conflict



and uncertainty than the Set problem itself (Epstein 1971). In summary, however, we can state the following:

- 1) Greyhound-like dogs are known at their earliest in both Egypt and Mesopotamia in pre-dynastic times, and are felt to have been originally bred in one area or the other.
- 2) Theories of descent have been described from *Simenia simensis*, the Abyssinian wolf, from *Canis aureus lupaster*, the golden jackal, from *Canis lupus pallipes*, the Indian wolf, or from Egyptian Pariah dogs. None of these theories seem fully satisfactory, yet all point to some cross-breeding of "local" canines, usually of a wild sort.
- 3) Ancient desert rock art shows humans accompanied by elongated hunting dogs with long tails and prick ears, indicating that greyhound-like animals were domesticated around northern desert regions in prehistoric Egyptian times (Epstein 1960).
- 4) The earliest pictures all show the dogs with "ringed" tails, which the Egyptians tell us were bred into the dogs. We even know that the name of this type of dog was *tesem*, and it became widely valued in North Africa. We also know that a lot of breeding effort was needed to incorporate a curl into the ancestor's straight-tailed nature (Epstein 1971).
- 5) A specific "modern" fact is that the East African Middle Nilotic greyhound called the Shilluk seems to express several Set-like and possibly primitive physical characteristics (fox-red coat with black muzzle, five front toes but only four back ones, ears pricked but drooping at tips, long robust tail broadening at tip).

So, one Set candidate could have been a highly dangerous canid, morphologically related to the greyhounds, but with certain physical and behavioral differences bred out by the Egyptians.

The problem areas involved are: a) Set has certain non-greyhound behaviors (odor tracking; nocturnalness; man-eating viciousness); b) the hooked nose; c) the erect tail; and d) the peculiar ears. Regarding a), the viciousness can be adequately explained. The ancients were in constant fear of being attacked by their own hounds. The primitive ferocity of the meat-hunters was often too evident (Merlen 1971). The problem of a greyhound ancestor having good odor tracking and a willingness to prowl at night can also be explained. Most wild canids have these qualities. Any mixing of genes to produce the primitive greyhound could have easily involved an animal with these common attributes. The other three areas present more of a difficulty.

The hooked nose: There are a few useful facts which have been recorded which may point to a solution of this odd Set-like feature. First, there is a wide margin for variation of muzzle lengths even within a single species.

*Canis familiaris*, for instance, typically shows neotenuous characteristics of shorter jaw and snout lengths than its wilder predecessors. A greyhound and its predecessors might differ similarly. For reasons no one fully understands, wild animals, such as wolves, show longer muzzles and greater variability in size than those born in captivity (Epstein 1971). Also, southernly wolves, though smaller overall, have longer muzzles than northernly ones, and animals of the same species have different body builds and facial proportions depending upon the latitude of their particular ranges. The point is that wild canids have quite a large actual and potential variation in muzzle length and jaw structure. Some varieties have evidence, admittedly unusual, of a convex rather than concave or straight splanchnocranium (the bone anterior to the neuro-cranium which could give the profile a downward curved aspect). Also, small changes in the tissues or hair color overhanging the bone structure can give a curved appearance, as is sometimes true with the Borzoi breed of dog, for example.

Ancient breeders have also let it be known that their hounds had this "hooked nose" quality buried, rather shallowly, in their genetics. The Greek historian, Xenophon, reporting on hound breeding in his time, noted that, when the two most famous breeds, the Alopecid and the Castorian were crossed, a variety of forms arose. A common type, built high-in-the-leg and with a hooked nose, was not valued and was often destroyed (Merlen 1971). Also, a pictograph from the Mesopotamian city of Uruk shows a greyhound-like animal with a curved muzzle and an erect tail. To this author, the Greek testimony is conclusive as to the curved muzzles of some ancient hound relatives.

The erect, tufted tail: Although modern greyhounds often have "rat-tails" without tufts, this is not always so. The Shilluk, as we have mentioned, broadens the tail at the tip, and a recent example from Aqaba showed a bushier tail and prick ears. In both the rock art of the Sahara and the depictions of predynastic Egypt, the dogs were shown with ringed tails, which were deliberately bred into the animals. We know that the primitive wild tail, therefore, was quite different from that desired. The Egyptians may have been deliberately breeding out a symbol of wildness and danger. Theorists suggest that looking for the curve of the tail was the quick method of evaluating how wild the dog was. Today, we can still see the "alpha" or dominant wolf in a pack raise its tail vertically as a display of its pre-eminence (Hall and Sharp 1978). One drawing from old Egypt shows a short-muzzled hound sitting, but still with an erect tail. Maybe it was an artistic error, maybe not (Epstein 1971).

The peculiar "squared" ears: The Egyptians not only did not like the erect, straight tail of the hound, but they did not like the primitive ears either. They tell us this by their paintings across the dynasties: the erect prick ears

gradually disappear, until they are never seen in later times. It therefore seems that they actively bred out the primitive ear conformations as well. Variations still occur, however. On Ibiza, a hound is described with "peculiar square-shaped ears and a convex splanchnocranium." In Bantu society, a prick-eared greyhound is almost identical to the early Egyptian variety. Even more intriguingly, both the Bantu and Egyptian dogs had their ears cropped. Why would this be? The Egyptians are silent, but the Bantus explain: it is to protect them against rabies (the spluttering saliva of Set?).

Before concluding, we should discuss another possible candidate for Set: the hyenas. These animals have many Set-like qualities: strength, aggression, viciousness, prowling, nocturnal life, possible correct coloration and "black-face," carnivorous, carrion-eater, body stealer and dismemberer, will attack in numbers, tracking by odor, possessors of a vile smell, and marking territory in sitting position with tail erect (Cloudsley-Thompson 1967). The Bible word *tsiy* seems related to a longer Hebrew word meaning hyena (United Bible Societies 1972), and some skins allegedly belonging to the Nandi Bear have turned out to be those of brown hyenas (Kingdom 1977).

A primitive or proto-hyena is a somewhat better possibility, and a bit of commentary about hyenid evolution is appropriate. The evolution of the hyenids is not well understood. Many forms are only very partially preserved in the fossil record, and the species tend to be differentiated primarily on the basis of tooth structure. This, however, is of interest to our subject, for tooth structure indicates "lifestyle," possibly even a more or less Set-like lifestyle. This is because, while most hyenids were adapted to scavenging (with bone-crushing teeth), some, like modern-day *Crocota*, were at least partially adapted to an active predator role (with meat-slicing teeth). Could an early form of *Crocota*, or a *Crocota*-like hyenid have terrorized Egyptians in pre- and proto-historic times, and perhaps still survive today as the basis for some of the Nandi Bear tales?

The origin of *Crocota*-like hyenids in Africa is in debate (Ewer 1967). Some believe that the species originated in Africa. Others believe that it, or a precursor, radiated from Asia (which would have made Egypt its first African home). Evolutionary ecologists point out that the presence of large numbers of individual scavengers (such as jackals, hyenas, etc.) requires the presence of efficient predators to provide the kills. Did the felids of North Africa provide such numbers, or was another predator, perhaps the Set animal—a predatory hyenid—also active?

Recent fossil finds (Berta 1981) make the predatory hyenid possibility stronger. A North American species, *Chasmaporthetes ossifragus*, has been described with several characteristics more Set-like than hyenids existing today: dentition indicating an active predatory lifestyle, elongated (more dog-like) body build, indicating great running ability, generally more powerful musculature, generally large size, and a relatively long muzzle with

evenly sloping snout. *C. ossifragus* has been compared to the extinct European hyenid, *Euryboas*, a possible early invader of (or transplant from?) the African continent. If there were *Euryboas* or similar type hyenids in the Egyptian area in pre- or proto-historic times, they would make formidable candidates for Set. Unfortunately, *Euryboas* as it is known, possessed a shorter muzzle than *Chasmaporthetes*, but perhaps variations could be more Set-like in this characteristic. In truth, as yet unknown hyenids with even greater Set-like resemblance may have existed now that the American work has shown us some of the extent of possible variation. Furthermore, most Nandi Bear reports seem more hyenid in details. The hyena as generally described above could easily be named Set, and it continued to inspire dread in Medieval Europeans, who painted it with a split, tufted tail, and compared it with the Devil (White 1960). Even so, in this author's mind, the body-build of the Set animal as represented by the ancients makes some form of canid the preferred choice, and the hyena remains a secondary candidate.

#### CONCLUSIONS

After reviewing the pertinent evidence, we can reduce the possibilities to four when attempting to identify the Set animal:

- 1) A greyhound predecessor native to the desert regions of Upper Egypt, and possibly ranging further south.
- 2) A "misfiring" cross-breed produced by the Egyptians from other primitive wild canids.
- 3) Some oddly formed hyena or hyena-like species.
- 4) A unique (in Egyptian mythology) synthesis of qualities of various desert animals (aardvark, wild dog, hyena).

This author prefers the first possibility. The fourth possibility is unsatisfying in that all early illustrations of Egyptian gods are of real animals. Why Set should be the one exception is difficult to rationalize. The third possibility has some attractions, but African hyaenas are so different in basic body-build from the animal commonly illustrated as Set that the "odd hyaena" species would need to be very odd indeed. The second possibility is awkward in the author's mind, since it requires the Set animal to be the artificial selection product of human designs, albeit an undesirable one. It is difficult to imagine why Egyptian breeders would allow enough of such genetic misfires to live and subsequently become a dangerous element in Egyptian life.

Possibility number one, a greyhound predecessor, is very attractive in almost every aspect. The images of Set and the structure of ancient greyhounds are identical in basic body build. Many "personality traits" of Set are seen in wild canids today. The odd structural characteristics of face and tail are possibly rationalizable as extreme elements of forms seen in modern canids and in ancient pictures and descriptions of their predecessors.



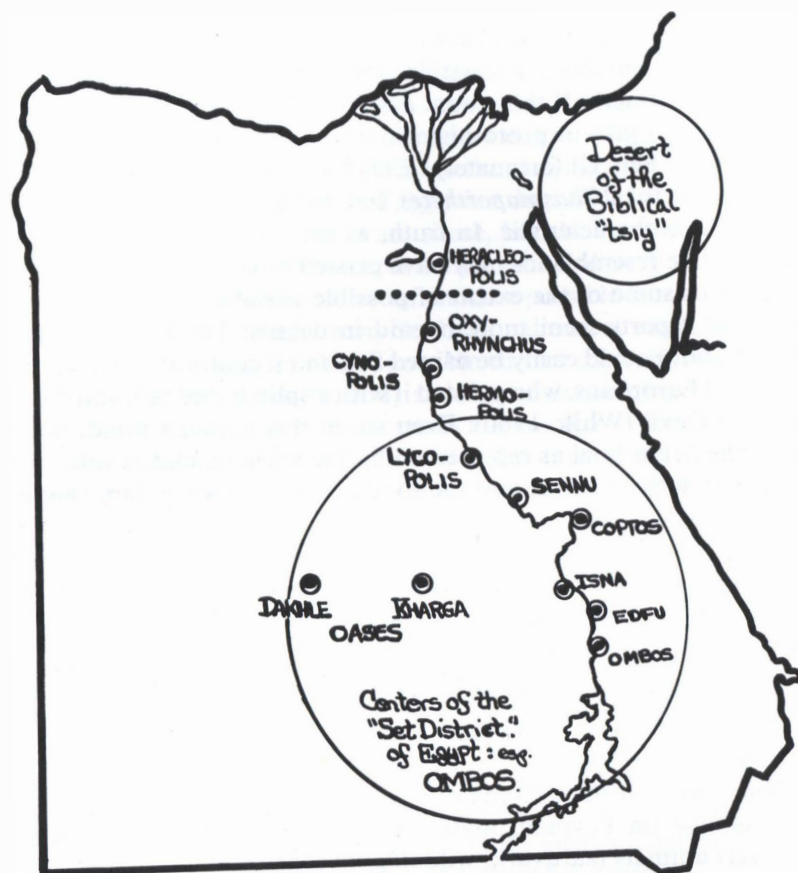


FIG. 5.—The ancient range of the Set-animal as speculated from the traditional worship sites and supplementary information (i.e., Lycopolis was allegedly the place where an Ethiopian army was driven off by a band of "wolves" in the desert).

The problem with the canid hypothesis is familiar to all cryptozoologists: we do not have the animal; we do not even have the bones. However, considering that modern reports of unclassified animals somewhat resembling Set occur in both East-central Africa and in the stony deserts west of the Nile, perhaps someday we will. The accompanying map (Fig. 5) was prepared on the basis of the clustering of the traditional worship sites of Set, as a hint of what at least its ancient zoogeographic range may have been.

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## TOWARDS AN ETYMOLOGY OF MAORI *WAITOREKE*

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**ABSTRACT:** A supposed New Zealand otter-like animal was known to the Maori as *waitoreke*. The meaning of this term is thought to derive from *wai*, "water." A number of alternate meanings, linking the animal with the mythological fauna of the Maori, are presented and shown to be equally, if not more, plausible. Another name for the animal, *kaureke*, is also examined.

### INTRODUCTION

The rarity and infrequent sightings of an animal known to the Maori as *waitoreke* have left investigators little to go on. After gathering the most complete collection of evidence concerning this "otter," Watson (1960) came to the view that "there is very little ground for any belief in the animal's existence."

According to Watson's sources, the first European sighting of the *waitoreke* was in May of 1773 by some of the members of the *Resolution's* crew; the *kaureke*—of which more later—was first seen by whites in 1844, four years before Mantell heard of it. It is thought that both names refer to the same animal, a jackal- or fox-like quadruped, about two feet long from the nose to the root of its bushy tail. Its head is said to look like a compromise between a dog and a cat. Its legs are short and thick. The fur is a grizzly brown or mouse color. There are apparently two forms: a terrestrial variety, which lives in a burrow and eats lizards, and an aquatic, fish-eating form, which lives in a beaveresque house it builds along lakes and streams. These houses are said to have one opening above the surface and another below. The house sometimes floats freely. The *waitoreke* is active at dawn and dusk. When frightened, it emits a scream and gives off a strong, musky odor. The tracks are said to resemble those of the European otter, although the toes are not webbed. Details of its morphology are unclear and even contradictory. According to the same sources, one native account stated it lays duck-sized eggs, while another stated it did *not* lay eggs, but neither did it have a pouch. Natives also claimed they were kept as pets in olden times.

When he took into consideration the evidence of the reports, along with the unlikelihood of either trans-oceanic dispersal or a Cretaceous land bridge from Australia to account for the quadruped's presence in New Zealand, Watson came to his negative conclusion with only the slightest reservations. Since the etymological evidence presented by Watson and also by Ley (1959) was quite brief, this paper assembles the available linguistic data concerning the *waitoreke*.

### NAMING PROCESSES

Processes of naming animals are quite complex, but we can point to several general techniques: 1) An animal that is well-known to a population has a name in the native language, although the etymology often is unknown even to native speakers, e.g., "horse." 2) An animal introduced by foreigners usually retains the foreign name, e.g., *zebra*. 3) An animal is entirely new and is undesignated in both native and foreign speech. In this case, the process begins when the speaker describes the encounter to someone else, and this continues until a descriptive neologism is coined to refer to the more lengthy narrative, e.g., "French moose," a Canadian Indian term for horse. If the encounters are repeated, consensus refines the neologism. When encounters are rare, several names may develop. 4) With well-known creatures, an allegorical name can arise, but this is usually in addition to its primary name, e.g., "man's best friend," "ship of the desert."

With these considerations in mind, an etymology of *waitoreke* should reveal information about the speaker's relationship with, or impression of, the animal. Due to the animal's rarity—both in the wild and in print—allegorical naming, 4) above, can be ruled out.

Observing the characteristics of Maori will facilitate judging whether or not *waitoreke* has been borrowed from another language. The phonetic repertoire—those elementary sounds of speech—is quite limited in Maori. Only fifteen are employed (*a, e, i, o, u, h, k, m, n, ng, p, r, t, w, wh*), and a restricted number of phonemic patterns are available (Krupa 1966). Since no word can end in a consonant, clusters of consonants are not allowed, and no consonant can occur by itself; the permissible word canons are either mono-vocalic (CV or V), bi-vocalic (CVCV, VCV, CVV, or VV), or poly-vocalic (CVCVCV, VCVCV, CVCVV, or VVV). There are also some further constraints on possible phoneme sequences (Biggs 1961) which need not trouble us here.

The necessary phonetic alterations a borrowed word may undergo, depends primarily upon how well the original word maps onto the Maori phonetic system. Only orthography may change, i.e., no phonetic change (*kau* "cow"), a final vowel may be added (*kuri* "cur, dog"; *kawhe* "calf"), a consonant cluster avoided (*maki* "monkey," cf. Malagasy *maki* = *Lemur catta*), or some combination of these changes employed (*poaka* "pig, pork"—not *poroka* which means "frog"). When there is no Maori equivalent, allophones are used (*raiona* "lion"; *tia* "deer," cf. *pia* "beer"), or the same Maori phoneme is used for a class of foreign sounds, e.g., *h* for all fricatives except *f* (*hipi* "sheep").

Since no plausible foreign word has been suggested to account for *waitoreke*, and in view of the morphemic compatibility of the word and the Maori language evident from the dictionaries (Savage 1962, Williams 1971),



we must assume that we are working with an established Maori word or a recent neologism constructed on Maori grammatical patterns.

A very limited number of words for mammals betray any true Maori origins. The term for "dog," *kararehe*, is really just a more general term for "animal" or "beast"; *ngeru* "cat" is, however, of Polynesian origin. Treager (1891) suggests that it is perhaps a modern or foreign word. Considering Samoan *geli* "cat," Tongan *geli* "a monkey," and Tahitian *eru* "scratch," it seems likely that, in this instance, the Maori morphemes were at work rather than borrowing. The word is native, even if the animal is not. Among land mammals, the only ones native to the Austronesian environment are the *kiore* "rat" (cf. Hawaiian *kiole*), and the *pekapeka* "bat"; *kiore paku* "little rat, mouse" is modern. A number of names for whales and porpoises are native: *pakake* (the Minke whale, *Balaenoptera acutorostrata*), *tohora* (a right whale subspecies, *Balaena glacialis australis*), *paraoa* (the sperm whale, *Physeter macrocephalus*), and *wai-aua* (Hector's dolphin, *Cephalorhynchus hectori*).

The Maori awareness of animals was quite different than that of most modern non-Polynesian cultures. Animals we might consider very different could be called by the same name; *ngarara* means "an insect" and also "a reptile"—one of which was considered mythical (McLintock 1966). There is also a Polynesian tendency to use cognate or similar terms for shaggy or broken surfaces. For instance, *huru* is equally valid for "feathers" or "hair." There are, certainly, similar situations in other languages; for example, the medieval English "vermin" referred to everything that crept or crawled, in the sense that many persons today use the term "creepy-crawly." There is the possibility that this "hair/feathers/scales" equality has significantly clouded the issue.

#### THE ROOTS OF WAITOREKE

Turning more specifically to *waitoreke*, we can begin as Ley (1959) and Watson (1960) did by examining the likelihood that the meaning somehow derives from *wai*, the general term for "water."

There are, in fact, a number of water creatures whose names are in all probability derived from *wai* "water." These include *ha-wai*, the term for a kind of fish, "the black kokopu" (*Galaxias alepidotus*), "a bullhead" (*Gobiomorphus gobioides*), or "an edible tree fungus"; *wai-keo* or *wai-ehu* "a kind of fish"; *wai-kaka* "mudfish" (*Neochanna apoda*); *wai-ngenge* "a kind of shark"; *wai-whakaiho* "a small crab used as shark bait"; and the aforementioned *wai-aua* "porpoise."

The meaning "water" is evident in an abundance of other terms. These include those which refer to bodies of water (*waipeka* "branching water," *wairoto* "enclosed water, lake," *waiariki* "hot spring, pool," *hawai* "water course, rivulet, shallows of a lagoon or swamp," *waipuke* "flood"); kinds of

water (*waitai* "saltwater," *wai maori* "fresh water," *wai reka* "sweet drink," *wai piro* "strong drink"); and the action of water or its results (*moo-wai* "to become wet," *kuu-wai-wai* "wet," *a-wai* "heavy, sodden," *waikura* "[to] rust").

This root is also frequently used in place names, such as *waipounamu* "Greenstone Waters," and *hawaiki-pumamao* "Traditional Homeland of the Maori." A very probable usage of the *wai* morpheme is found in *waiu* "milk." Still others indicate possible connection: *puwai* "flower, blossom," *waimarie* "luck, fortune," *waia* "be accustomed," *waihanga* "to build." A seemingly likely candidate, *waina* "wine," is a borrowing; and *wairo* from *waiero* "dog's tail, hair of a dog's tail," is very unlikely to be related to water.

As convincing as this wealth of words relating *wai* to "water" may be, the situation is far from straight-forward, especially with place names. In all languages, the meanings of place names become obscure over a period of time. Thus, in Maori *waipa* may mean "the river of fortified villages," but the meanings of *waipawa* "river/town," *waikouaiti* "town," and *waiheke* "island" are far from clear (McLintock 1966). So, even though place names contain the root *wai*, their value as data is open to discussion.

More relevant to *waitoreke*, in very few cases where the phonemic pattern *wai* unambiguously means "water" is that pattern followed by the Maori dental phoneme *t* plus the back vowel *u* or *o*. In every other case where it is followed by *-to-* or *-tu-*, its meaning is related to witchcraft or religion.

Two exceptions can be cited. The first is *waitomo* "caves," where the name translates best as "water passing through a hole." Waitomo Caves are indeed a magical place:

It is a spectacular sight to see the glow worms that live in the caves in New Zealand, the most famous being at Waitomo, about 200 miles north of Wellington. The ceilings of these caves are covered with thousands of glowing larvae, and from each is suspended a long luminescent thread that apparently serves to catch food particles or small insects. If one talks loudly, or if the wall of the cave is tapped sharply, the larvae turn off their lights virtually as one. After a brief period the lights come on again, tentatively at first and then more boldly, until the whole ceiling is once again ablaze [McElroy and Seliger 1962].

As tempting as it might be for our purposes, however, there is no evidence that any Maori ceremonies or magic took place in these caves.

The second exception is *waituhi*, which Williams lists as having several meanings. Used as a noun, it means either "a pool of water collected in a hollow tree or rock around which bird snares are set," or "a freshet, first signs of flood waters." But used as a transitive verb, it means "to perform birthrites" (see below). Used as an adjective, it means "red." These may not, however, all be the same word.

It is very likely that linguistics is becoming entangled in the Maori socio-religious complex, and to assume that all words containing *wai* relate only or primarily to "water" is thus not justified. Even a brief review of the Maori

spiritual or magical data in which *wai* is evident, makes a convincing argument: Another meaning for *wai* is "memory, recollection of words heard," detected in *paki waitara* "legends, myths, beliefs," and in *waita* "song, to sing." A considerable number of words and expressions derived from this meaning are related to the spirit world: *wairua* "spirit, shadow, soul"; *tae-a-wairua* "be there in spirit"; *hono-i-wairua* "Gathering Place of the Spirits"; and *whaka-wai(a)* "entice, beguile, (tempt)."

We know that the spirit world played a very important role in Maori society at the time of the first European contacts. Hogg (1980), in his work on cannibalism, quotes a Dr. Felix Maynard:

There is not a bay, not a cove, in New Zealand which has not witnessed horrible dramas, and woe to the white man who falls into the New Zealander's hands. When the victor eats his foe after combat, he believes he eats not only his body but his soul. It is an outrage to eat the body; and it is an advantage to eat the *waidoua*—the soul of the vanquished—because this is then assimilated with one's own. This superstition is all-powerful in war-time.

(Dr. Maynard most likely rendered the Maori flapped *r* as an intervocalic *d*. The word should be *wairua*.)

In Maori religion and folklore, *waitokorau* was a kind of witchcraft. The personification of witchcraft was *makutu*. He dwelt with and served *miru*, the wicked goddess of the gates of death, in *tatau-o-te-po* "Door of Night." Thus, it is possible that *waitutu* "dark in color" is related to the magical "darkness of night, or death."

A Maori legend concerning two rivers seems to confuse the issue by relating water to the spirit world, but perhaps relates "water," "essence," and "spirit":

According to the story the *waiiau-uha* (Waiau) and the *waiiau-toa* (Clarence) were respectively male and female spirit lovers living in the Spenser Mountains. For some reason they were transformed into rivers, the sources of which were not far apart. When warm rains melted the snows and caused floods, it was said that the parted lovers were lamenting and that the rivers were swollen with their tears [McLintock 1966].

In recent Maori, a number of words show clear relation of the (w)aito-morpheme with the magico-mythological concept. We can cite *aitu(-tia)* "death, misfortune," and *aitu-* "ghost," from Proto-Austronesian *\*hantu* (Treager 1891). (The present reconstruction is *\*qanitu*, but this entails no great difficulty as the *n* is thought to have dropped out [Capell 1962]. Also, the initial phoneme, *\*q*, may or may not appear—cf. the pronunciations of English "huge"—without disturbing the etymology.) We should also include in the witchcraft complex *waituhi* "birthrites," *waitohi* "a kind of incantation used before combat," and *waitohu* "a mark, to mark an animal, reserve for oneself, prognosticate."

As to *waitoreke* itself, Pollack (Costello 1974) points to the South Island Maori dialect variant, [k] for [ng], suggesting *waitoreke* from *waitorengi*.

Compatible examples do exist, for instance *waitaki*, the southern variant of the northern Maori place name *waitangi* "Weeping Waters." Considering our findings above, however, the meaning of *waitorengi* would not be "disappearing under the water" (an interpretation Pollack himself feels is "a little too pat"), but perhaps "disappearing ghost/spirit." The difficulty of settling on a satisfactory etymology is further increased by another variant: *waito teke* (Watson 1960).

The North/South Maori variants essentially attest to the stability of *waito-*, but, thus far, no clear solution can be presented for the etymology of *-reke*.

A further possibility arises from an etymology based on the definite or indefinite pronoun *wai* "the one who . . ." This line of thought would yield meanings such as *wai-toreke* "one forsaken" (*toreke* "left behind, forsaken") and the North Maori *wai-torengi* "one-sinking" (NM *torengi* "sink into [as the sun into the horizon]"), or "water-sinking."

In this sense, John Colarusso (1985) has suggested the possible connection of *waitoreke* to North Maori *wai-rangi* and SM *wai-raki* "foolish, one dried up, wrinkled, a monster, a mythical animal." The term *wai-to-rehe* "one of wrinkles, skin marks" (*rehe* "wrinkles, tattoo marks over the eyebrows, wizened, stunted, puny") perhaps adds confirmation to the supposition. Both *waitoreke* and *wai-raki* "monster, mythical animal," would then be a variant of *wai-to-rehe* "one of wrinkles." We will see below that there is some evidence of the *-reke/rehe* variant.

Finally, there is the possibility that the name is not amenable to analysis. Sir Peter Buck felt (Krumbiegel 1950) that *waitoreke* was "quite ungrammatical." This pronouncement, although meaningless in linguistic terms, still carries considerable weight. Sir Peter's real name was Te Rangihiroa; he was the son of an Irishman and a Maori woman. He grew up among his mother's people, obtained his higher education in England, and eventually returned to New Zealand, where he began 25 years studying and recording the native cultures of the Polynesian Pacific (McLintock 1960). It seems prudent here, perhaps, to overlook his ignorance of modern linguistic terminology, and share his better-informed suspicion.

#### THE ROOTS OF KAUREKE

Before summarizing our efforts and drawing a conclusion, we should examine briefly another name recorded for this animal. The term is *kaurehe* or *kaureke*, the latter being the spelling Mantell—who first recorded the term—used in an 1850 article. But according to Watson (1960) he changed it to *kaurehe* in an 1851 report. It is not known whether he considered the first incorrect or simply a less preferable variant of the second.

Heuvelmans (1958) suggested the possibility that a completely separate creature was involved, since sufficient discrepancies in the animal's color-



ation were described. This position is supported by Watson's (1960) data. Ley's data neither credits nor discredits this point of view.

Like *waitoreke*, *kaureke* supports either a water or magical interpretation of the beast. The primary meaning of *kau* is "swim, wade," but another is "ancestor," or "the name of a mythological fish hook and line." Complicating the matter is *kaua* "an extinct native bird similar to the kingfisher"—obviously found near the water. Also *kaurehe* can refer to the *tuatara* (*Sphenodon punctatus*), as well as a monster. Watson (1960) noted Duff's argument that *kaureke* is "almost certainly a printer's error for *karara ke*, meaning 'a different type of lizard' (hence *tuatara*)." Duff is believed to have misread "coarse grisly hair" as "coarse gristly hair," and mistaken it for the spines of a reptile. The line of reasoning is therefore spurious.

(As has recently turned out [Clark 1984], there actually was within historic times at least one other large lizard in New Zealand in addition to the *tuatara*. In the late 1970's, the curator of The Institute of Natural History in Marseilles requested help from the British Museum in identifying a large stuffed reptile brought from New Zealand, probably by d'Urville. Work on the 60-centimeter-long lizard by Aaron Bauer, at the University of California, Los Angeles, reveals it to be a new species of gecko, believed extinct since the middle of the last century. Some hope is held, however, that this largest known gecko—about the size of the *tuatara*—is still alive in some of the rugged areas of the North Island. Two other large lizards are mentioned in the Maori mythology, the *kuai* [Stack 1874, Anonymous 1898, Hutton 1898, Watson 1960] and the aforementioned *ngarara* [Stack 1874, Watson 1960]. Mostly likely the gecko will turn out to be the *ngarara*.)

Watson also points out that "The Maori certainly acknowledge a large fauna of completely mythical beasts, monsters and man-like creatures with supernatural powers, of which the *kaurehe*, some sort of man-eating dragon, was one . . ." This naturally presents the possibility that the *kaurehe* was at one time the marine crocodile (*Crocodylus porosus*)—a perfect man-eating dragon. But it is quite difficult to imagine the term being applied to a two-foot-long furry creature that flees from man. We are perhaps etymologically safer in suggesting *kaurehe* as meaning "ancestor-wrinkled, wizened" or "water-wrinkled."

#### CONCLUSIONS

In summary, the following points should now be clear: 1) The roots *wai*, *waito*, *waitu* involve a panoply of meanings which includes but is not limited to "water." 2) Meanings associated with the root (*wai*)to relate to the spiritual, magical, or mythological world of the Maori. 3) The word may derive from the pronoun *wai* "the one who . . ."; one word (*wai-raki*) is even glossed as "monster, mythical animal." 4) No simple and satisfactory etymology

can be presented for the ending *-reke*. 5) *kaurehe* also affords alternate interpretations, relating either to "swimming" or "ancestors."

Certainly we have shown that *wai*, "water," is only one of several possible roots from which *waitoreke* could have come. But aside from returning to the mind-set of the Maoris who named the beast, it seems impossible to judge from the data whether or not this is a real or mythical animal. Among the wide range of possible meanings, a number of them would appear incompatible with the physical description of the animal. Linguistically considered, the author feels the *waitoreke* was a part of the Maori mythological fauna. This does not, it is true, accord with the fact that Europeans also saw a beast that the Maori called by this name. What the linguistic data suggests is that the name of a mythological creature was applied—perhaps mistakenly—to an animal with which the recent Maori were really as unfamiliar as the white man. The "python," named after the mythological serpent that haunted the caves of Parnassus, and the "Tasmanian devil" are both examples of old, mythological terms applied to recent animals.

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## THE CASE OF THE PYGMY GORILLA: A CAUTIONARY TALE FOR CRYPTOZOOLOGY

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**ABSTRACT:** The pygmy gorilla is a concept derived from confusions and misunderstandings originating in the early years of this century. The specimens on which the concept of a pygmy gorilla (*Gorilla mayéma*, *Pseudogorilla mayéma*, or *Gorilla* [*Pseudogorilla*] *elliotti*) were based are all perfectly normal gorillas. In one case, failure to appreciate the nature of age-changes in the skull were responsible for the erection of a taxon. A misreading of *mayema* for *manyema* gave the pygmy gorilla a habitat in eastern Zaire. Such confusions must be disentangled before well-founded cryptozoological research is undertaken.

### INTRODUCTION

One of the essential precursors to genuine cryptozoological research is the clearing away of dead wood. Searching for a species which, though it stands in the literature, does not exist and has no basis in reality, is a waste of time and distracts attention from more promising and more soundly based research.

In this paper, I will examine the basis of the belief that there is a pygmy gorilla waiting to be rediscovered after being lost for nearly three-quarters of a century. It has been cited as being "still controversial" (Heuvelmans 1984), and, indeed, has been the object of a special expedition to search for it (Raper 1945). Nonetheless, its initial description was entirely a product of the style of animal taxonomy prevalent in the early two decades of the present century: a combination of a species-splitting tradition and a basic biological ignorance whose character and all-pervasiveness is difficult to realize at this distance in time.

In the early 1960's, I amassed an enormous amount of data on cranial variation in the gorilla, measuring in the process 745 skulls of adult gorillas, of both sexes, as well as comparative subadult samples. I was able to examine and measure all type specimens in the genus with the exception of two (*Gorilla beringei* and *Gorilla gorilla schwarzi*), which are no longer present in their original collections and are said to have been destroyed during World War II.

By means of multivariate analysis, I determined (Groves 1970) that all gorillas belong to one species, *Gorilla gorilla*, with three well-defined subspecies. Aside from multivariate analysis, a series of univariate analyses were performed to determine expected ranges of variation in different characteristics.



It should be specified, finally, that to say that two geographically-based samples belong to one subspecies is not to say that they are identical. Strong average differences between geographic samples within at least two of the three subspecies are apparent: subspecies are merely those samples between which such strong differences exist that a majority of specimens can be correctly allocated.

#### ALIX AND BOUVIER'S PYGMY GORILLA

From the time of its first scientific description (1847) until the early 1870's, all gorillas whose remains found their way to scientific institutions (in the U.S.A., U.K., France and Australia) were from a restricted area in Central Africa, around the Gabon Estuary as far north as the present-day Rio Muni and as far south as Fernan Vaz. In 1877, a gorilla from much farther south, "the village of King Mayéma, on the banks of the Quilo at 4°35' South," was described by E. Alix and A. Bouvier as a new species, which they named after the king of the country of origin, *Gorilla mayéma* (Alix and Bouvier 1877).

The specimen, an aged female, was described as being smaller than the known gorilla from the Gabon, with various skull differences, less developed spines to the first three cervical vertebrae, shorter clavicle, more slender and less muscularly marked forearm, hand, lower leg and foot bones, and long-haired back.

Later, Famerlart (1883) corrected the type locality of *Gorilla mayéma* to Condé, near Landana, and reported that he himself possessed a living youngster of the same species.

Hartmann (1885) gave further notes on *Gorilla mayéma*. The small hands and slender limbs, in his opinion, pointed to a youthful, not an aged specimen; the rest of the skeletal features described by Alix and Bouvier were variable, both individually and sexually; the hair on the back is sometimes worn away, sometimes not. So, just eight years after its original description, the whole basis for the species was fairly conclusively explained away.

A skull in the Laboratoire d'Anatomie Comparée, Paris, number 9772, is labelled "Gorilla mayéma: Rode, 1944." It is unclear whether this means that Rode had discovered this to be the type of the species, or whether he had merely identified it with the putative species. It is, in fact, consistent with the type, and, considering that *Gorilla mayéma* is not a species which appears in Rode's published writings, the former explanation could actually be correct. It is truly a very small specimen, only 206 mm in total length (prosthion to inion). Other female skulls from this southerly district (Mayombe, Cabinda, southern Gabon) average 236.5 mm, with a standard deviation of 10.08 ( $n = 4$ ), and the mean for the whole Gabon region, south to the mouth of the Congo, is 227 mm (S.D. 8.69,  $n = 27$ ). The next smallest

skull is 213 mm long. A skull of length 206 mm is therefore to be expected in such a sample, being somewhat over 2 standard deviations from the mean.

#### ELLIOT'S PYGMY GORILLA

The publication of the third, final volume of Daniel Giraud Elliot's epoch-making, much maligned *Review of the Primates*, occurred at the start of World War I (Elliot 1914). By this time, the "opening up of Africa" had revealed a host of previously undescribed mammal species, most of them procured in the first instance by the big-game hunters who followed hot on the heels of the missionaries and colonial administrators (or even, in some cases, preceded them). The relatively sudden influx of new specimens into museums, in Britain, Germany, and the United States in particular, was more than some of the mammalogists could handle. Used to comparing one specimen of species A with two of species B, they tended to think in terms of taxonomic variation rather than individual, age-dependent or sexual variation; this resulted in a host of spuriously "new" species being described along with the more modest list of genuinely new ones.

In his monograph, Elliot (1914) both suffered from and contributed to this prolixity. He found it difficult to make sense of many of the species described by his contemporaries, and his attempts to divide them up into genera and species-groups were, in the main, disastrous. His text is replete with remarks displaying admirable caution, which he would then, all too often, fail to live up to. In the case of gorillas, he had to wrestle with the problems created by Paul Matschie, of Berlin, perhaps the most reckless splitter of species of them all; he had a theory about the regularity of distribution patterns among mammals, and new species or subspecies would miraculously appear in his works in answer to the *a priori* need for a special local form. So it was that Elliot expressed skepticism about the gorilla species and subspecies that Matschie had described, and was still in the process of describing; but he then proceeded to burden the literature with a far more baleful error than any of Matschie's, and one whose insidious effects are still with us today.

Having searched for the type of *Gorilla mayéma* Alix and Bouvier in the Paris Museum, and having failed to find it, Elliot applied the name, giving no justification for his action, to a collection of three specimens (male, female, and young) in the Senckenberg Museum, Frankfurt. But he went further. Being under the impression that the skulls were intermediate between gorillas and chimpanzees, he removed the name *mayéma* from the genus *Gorilla* and erected a new genus, *Pseudogorilla*, for it. At the same time, he did admit that there was no real guarantee that his *Pseudogorilla mayéma* was in actuality the same species as Alix and Bouvier's *Gorilla mayéma*; anyone thinking they were different would, he noted, be at liberty to rename his (Elliot's) species.

The description of the new genus is, in essence, as follows: size small, but somewhat larger than the adult chimpanzee; braincase similar to the chimpanzee's, being large, full and rounded, sagittal and occipital crests wanting; forehead prominently rising above orbital ridge; a rather broad, flat expansion from occipital region to root of zygoma; face in profile having a slant of 45° from orbital ridge, rostral portion protuberant, narrow, lengthened; anterior portion of zygomatic arch at its root only reaches the anterior edge of first molar; lower horizontal line of mandible rounded, not straight. Distribution of hairy covering like *Gorilla*.

Elliot remarked that the prominent forehead and absence of crests is very much like in the chimpanzee, but the rest of the cranium is gorilla-like: shape of occiput, narrow face, form of mandible, and so on. The position of the zygomatic root is, however, also chimpanzee-like. The colors of the mounted skins are gorilla-like. The male skin is 1,350 mm high, and the male skull 220 mm long.

One of the most useful aspects of Elliot's monograph is the quite excellent black-and-white photographic plates of skulls, representing all genera. A glance at the photo of the male *Pseudogorilla mayëma* shows that it is not fully mature: all the teeth are erupted, but the third molars are unworn and the basilar suture is unfused. Examination of the actual specimen, still in the Senckenberg Museum, confirms this. At such an age in male gorilla skulls, the sagittal crest has not even begun to form. As the third molars come into wear, the temporal muscles continue to increase in size, and the temporal lines on the braincase (marking the upper limit of the origin of the temporal muscle) become more and more prominent and move higher and higher, until—at about the time that the basilar suture begins to fuse—the ridges meet along the midline of the top of the braincase and begin to form the sagittal crest. The temporal lines move not only upward but backward as well, until they meet the nuchal lines (upper limit of attachment of the muscles of posture at the back of the head), and another compound crest, the nuchal crest (Elliot's "occipital crest"), is formed. A very few male gorillas never develop a crest at all; of the 469 male skulls in my study, four did not have a crest.

So, is Elliot's male *Pseudogorilla* merely a young male of an ordinary gorilla? There is absolutely no doubt of it. Elliot's skull length measurement was taken between uprights; my measurement, direct from prosthion (anterior point, between the upper central incisors) to opisthocranium (furthest back point), is 243 mm. The normal skull length of adult male gorillas is 280–330 mm; that of young adults with a crest in process of forming is 268 to 316; of young adults with an incipient crest, 259–280; of young adults with no crest, i.e., in the growth stage of the male *Pseudogorilla*, 222–275 mm. Elliot's male specimen is thus right in the middle of the normal range.

Elliot did not state exactly how he held the skull to estimate the position

of the root of the zygomatic arch; I held it with the alveolar margin of the toothrow horizontal and dropped a perpendicular from the infraorbital foramen (about at the position of the zygomatic root), which fell at the midpoint of the second premolar. This position is seen in many gorilla skulls, of all ages.

It is easy to ask how Elliot could have made such an elementary error. The only reasonable explanation must be that he was unfamiliar with growth patterns, and, indeed, the material available to him might not have included any other skulls of that age at all. It is even unclear whether he was aware that sagittal and nuchal crests are mechanical responses to muscular development, rather than genetic properties of particular species. Nor is there any indication in his text that he was aware that the skull in question actually was young, or that he knew about the basilar suture criterion.

What, then, of the female? Female gorillas, or 70% of them, lack sagittal crests, and have, at most, poorly raised nuchal crests. The skull length of the *Pseudogorilla* female is 220 mm, which, as was shown in the previous section, is normal for an adult female western gorilla—which is what it is.

There is no reason to disagree with Miller (1915: 6), who stated only a year after Elliot had written: "The genus *Pseudogorilla* was based on two specimens of true *Gorilla*, an immature male with all the teeth in place but with the basal suture open and the temporal ridges separate (l.c.pl.32), and a mature female with the basal suture closed and the temporal ridges joined (l.c.pl.33)."

Elliot gave us the locality for his new taxon "Upper Congo"; but the labels on the skulls clearly say "Fernan Vaz." This is a town halfway down the Gabon coast, and, for what it is worth, about midway between the type localities of *Gorilla gorilla* and *Gorilla mayëma*.

In view of subsequent events, it is sad that Miller's remarks were not more widely noticed. Frechkop (1943), normally the most cautious of zoologists, did exactly what Elliot had anticipated: Elliot's taxon was not the same as Alix and Bouvier's, so the name *mayëma* was not applicable to it, and it needed a new name. Regarding *Pseudogorilla* as valid only subgenerically, Frechkop therefore felt obliged to propose "pour le gorille-nain" a new name, *Gorilla (Pseudogorilla) ellioti*. It goes without saying that, as a replacement name for *Pseudogorilla mayëma* of Elliot, this name too falls as a synonym of *Gorilla gorilla gorilla*, the western lowland gorilla.

#### PYGMY GORILLAS IN EASTERN ZAIRE?

There have been a few other mentions of *Gorilla mayëma* in the literature, and these too have contributed to the confusion. Rothschild (1905) stated boldly: "The *Gorilla manyema* [sic] of Alix and Bouvier I believe to be a very large ape of the group of *Simia vellerosus* Gray, and not a gorilla at all." (Chimpanzees, like gorillas, had been split up by Matschie and others



into a number of different species; *Simia vellerosus*, a mid-19th Century creation, had been revived to take its place among all the other "new" chimpanzees.) There are two points of special interest in this flat claim: first, Rothschild thought the Alix and Bouvier species was a chimpanzee, not a gorilla; secondly, he misspelt the name. Manyema (or Maniema) was a district of the then Belgian Congo, now Zaire, east of the Lualaba River: evidently, Rothschild had carelessly misread *mayéma* as *manyema*, jumped to conclusions as to where the type locality must be, and ascribed it, being described as small in size, to a chimpanzee. Chimpanzees were at that time already known from the Lualaba district; gorillas were not.

Three years later, Rothschild had changed his mind (Rothschild 1908). He had since received specimens of "Alix and Bouvier's *Gorilla manyema*" [sic!], and he then saw that this was not a chimpanzee after all, but must be the gorilla race of the "South Congo." What he meant by South Congo is ambiguous, but from his consistent use of the lapsus "*manyema*," it is probable that Maniema is meant; indeed, there is in the British Museum (Natural History), in London, a skull from the Rothschild collection, no.1939.945, on which is written "*G. g. manyema*. Upper Congo." It is a specimen of the so-called eastern lowland gorilla, *Gorilla gorilla graueri*, which does indeed live in the present-day Maniema National Park.

So, Alix and Bouvier's little gorilla had changed both its name and its habitat. It had changed its size, too, for Rothschild's specimen is a perfectly normal-sized adult male. And this additional confusion was confounded further in 1945, when an expedition was mounted to look for it.

In his report on the search, Raper (1945) spoke of *Gorilla manyema*, an ape living in the Manyema district north of Lake Tanganyika, "of which there is a specimen in the Frankfurt Museum." It was said to be the same animal as that described by Livingstone as "Soko." These apes, said Raper, were recorded in 1942, north of Kigoma, twenty miles east of Lake Tanganyika, but turned out to be chimpanzees (just as was Soko, incidentally). Raper's group found no dwarf gorillas there: it would therefore seem to have disappeared, or else it is sitting somewhere inaccessible, laughing at people's attempts to find it.

#### CONCLUSIONS

This paper has, I hope, been more than just the sinking of a redundant name into synonymy. Strictly speaking, such an action was in any case unnecessary, having already been accomplished (if in some obscurity) by Hartmann (1885) and by Miller (1915). I have tried to show something of the complexity of a spurious concept that will not die: how an ordinary error of taxonomic judgement in the 1870's changed its meaning, twice, independently, in the early 20th Century, and how the two altered conceptions came together again to create a creature that never existed. I have tried to show,

too, how we should view our predecessors' taxonomic work. In the last century, they had few specimens, and could not form proper judgements about the limits of individual variation; in the early years of the present century, they had too much material, all at once, but no framework in which to fit it—no real notions of distribution patterns or field biology, and little or no idea of what to expect about growth patterns. They were competent zoologists in their way: Matschie's writings, in particular, often contain startling insights, and are always full of interesting, if off-beat, ideas. But the early 20th Century was a watershed between two eras: Elliot carried with him the baggage of the 19th Century; Miller—just as much of a taxonomic splitter in his way—was able to cut through the untamed prolixity and see the biological reality behind the traditional taxonomic formalities.

That the confusion surrounding the "pygmy gorilla" concept had cryptozoological consequences is, really, no surprise. A genuine science of cryptozoology will first try to clear away such confusion, and see if there is any reality behind it all. In the case of the pygmy gorilla, I regret to state that there is none.

Though Heuvelmans (1984) has accused me of "overt hostility towards cryptozoology," which I hope the present article demonstrates to be in error, he has also paid me a spectacular compliment. I can do no less than return the compliment. In his book, *On the Track of Unknown Animals* (Heuvelmans 1958) (I cannot speak for the original French version, which I gather is not quite the same), Heuvelmans employs much the same technique as I have here, and which I have somewhat conceitedly referred to above as the "genuine science of cryptozoology." Chapter 17 of Heuvelmans' book, on the Nandi Bear, is a particularly fine example: Heuvelmans analyzes the reports, reaches behind them to the reality, and disentangles the different strands. Like the Nandi Bear, the "East African Proteus," the "pygmy gorilla" changed its shape continuously until it acquired its own bogus individuality.

#### SUMMARY

The pygmy gorilla was based on *Gorilla mayéma*, a supposedly small-sized kind of gorilla described in the 1870's by Alix and Bouvier. It was redescribed in 1914, on a different set of specimens, and placed in a new genus. The 1877 animal was simply a rather small female; the 1914 ones were a normal young male and an adult female, the former misinterpreted as adult. Independently, the name began to be written *manyema*, and the pygmy gorilla was thus unconsciously relocated from Gabon to the Manyema district, eastern Zaire. It was searched for without success in that region in 1945. The sorry tale of confusion can be traced to early taxonomists' failure to appreciate individual, age, and sex variation in natural populations. Cryptozoology must always take into account the background of reports of un-

known or poorly known animals, tracing such reports back to their origins in order to rid the field of confusion which may well have subsequently muddled the water.

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## SOLE PADS AND DERMATOGLYPHICS OF THE ELK WALLOW FOOTPRINTS

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**ABSTRACT:** Sasquatch footprint casts from Elk Wallow (Walla Walla), previously examined in detail by Krantz (1983), are discussed with respect to the presence of a large sole pad. A sole pad is an important component in locomotion, and reconstructions of anatomy and gait must take it into account. Because large animals compensate for heavy weight principally through locomotor pattern, a large, unknown biped need not have enormous stresses acting on the moving lower limbs, but would have a gait different from living bipedal hominids. Anatomical reconstructions must also take this into account. Dermatoglyphic evidence, if abundant, can be used to reconstruct behavior. Hence, it is possible that future research utilizing prints and trackways of these large, unknown animals in the Pacific Northwest may yield more information about locomotion and behavior than is presently the case, if investigators are willing to entertain the possibility that the animals responsible for the prints may be unlike any known mammal in anatomy and locomotion. Krantz's (1983) identification of the prints as hominid, by virtue of an adducted hallux, is questioned.

## INTRODUCTION

In his recent paper on the Elk Wallow (Walla Walla) Sasquatch footprints, Krantz (1983) presents a detailed summary of the configurations of three footprint casts, paying particularly close attention to dermatoglyphic data. Skepticism as to whether dermatoglyphic patterns can be preserved on plaster casts is answered by Krantz's experimental proof that a human thumbprint can be transferred from skin to soil to plaster, and by the fact that criminal identification by dermatoglyphics from footprints imprinted in soil is acceptable procedure by police departments in two countries. Krantz then investigates the dermatoglyphic patterns of the casts, and concludes from this evidence that the makers of the prints were primates. Krantz further identifies the Elk Wallow prints as made by a specific category of primates, hominids (Krantz 1983: 53)—that is, members of the zoological family to which living humans and their extinct relatives belong.

In this paper, I shall examine the sole pad, a feature of the Elk Wallow prints which Krantz does not discuss in as much detail as the dermatoglyphic evidence. This feature is, however, important for reconstructing anatomy and locomotion. I shall also examine the influence of body size on locomotion, and indicate some ways in which dermatoglyphic evidence, if more abundant, may be used to reconstruct elements of behavior. I conclude by



stressing the probability that the makers of the Elk Wallow prints are unique among mammals in terms of anatomy and locomotion, and that identification of the prints as hominid in origin, on the basis of a single trait (an adducted hallux), is incorrect.

#### THE SOLE PAD

One of the striking morphological features documented by the casts is the apparent presence of an extremely thick, flexible pad on the sole of the Elk Wallow feet. Given the impression of a stone in the middle of the "full right" cast, and a photograph of the actual print with stone in place, Krantz (1983: 64) estimates the minimal thickness of this pad as slightly greater than 2 cm in the middle of the sole. Anatomical evidence from the gorilla, largest of the living primates, indicates that a sole pad can exist with dermatoglyphic patterns intact on the plantar surface. It is therefore possible for a very large mammal to possess a thick, flexible sole pad with dermatoglyphics, and not merely ridged skin.

One might assume, by analogy with living mammals possessing such pads (Morton 1935), that the sole pad of an unknown mammal would be composed of fat and tough, fibrous connective tissue. The thick, fibrous strands would bind the skin to the deep fascia (plantar aponeurosis), and would form small compartments of firm and resilient fat. The depth of the pad might vary along the foot according to weight-bearing considerations. A sole pad is therefore a biologically normal structure, and one can use evidence from living mammals to infer its general composition in a form whose anatomy is unknown. Is it possible to estimate the depth of the sole pad in the Elk Wallow creatures using available information on sole pad thickness in living mammals? Such information exists for two primate species, gorillas and hominids.

Fat and connective tissue comprise the sole pad in the gorilla. Thick skin covers the sole of the foot, and is underlain by fat and fibrous tissue at the lateral border of the foot. The depth of this pad increases posteriorly, and reaches a maximum depth of 2.5 cm under the heel (Raven 1950: 71). In modern, bipedal humans, the sole pad is an important weight-bearing structure which is so compressible and shock-absorbing that a fall directly on the heel which shatters the calcaneus may leave no mark on the sole pad (Klenerman *et al.* 1976). The skin on the sole is thickened, especially at the heel, even in human fetuses, but the skin and the underlying pad of lobulated fat and collagen fibers is also subject to some variation in depth. Certain congenital diseases, accidents which result in the foot being placed in a cast, or prolonged bed rest may cause such thinning of the sole pad that walking is almost impossible. In acromegaly, this sole pad becomes very much thicker than normal, sometimes achieving a state "like a built-in layer of crepe rubber" (Klenerman *et al.* 1976: 137). Data on the average thickness of the

human sole pad at the heel region do not seem to be readily available. In dissections of cadavers, the skin of the sole is so thick and firmly bound to the underlying lobulated fat that it is difficult to disclose the plantar aponeurosis; I would estimate a thickness of about 2 cm in the human cadaver. Histological study of the subcutaneous tissue of the gorilla foot seems to indicate a composition and thickness similar to that of man (Straus 1950: 217). An average greatest sole pad thickness of 2.5 cm may therefore be nearer the modern human norm.

Note that the sole pad is about the same thickness in hominids and gorillas, although gorilla weight is approximately three times that of hominids. I now make two assumptions. First, I assume that bipedal locomotion, in which all of the body weight is supported by the hindlimbs, experiences more selection pressure for cushioning of the foot than is the case in quadrupedal locomotion. The thickness of the sole pad in the quadrupedal, knuckle-walking gorilla would therefore be increased in a biped of gorilla size. Professional human runners, who experience impact forces at heel strike of three to five times that experienced during walking (Roy and Irvin 1983: 422), are forced to control foot movement and absorb shock with specially constructed shoes and orthotic devices. In effect, they must create artificial sole pads in addition to the natural cushioning of the sole, and even then cushioning is not always adequate to prevent injury in running athletes. This is especially the case in marathon running, where impact forces acting over a distance often cause overuse injuries. Running bipedal hominids thus experience foot impact forces several times that experienced during walking, and often suffer as a result of inadequate natural sole pads, which have been evolved to withstand walking impact. I believe that this demonstrates that, if bipedal hominid body weight were multiplied several times—the equivalent of the increase in impact force experienced during running—the thickness of the sole pad would be correspondingly increased to ensure efficient bipedal walking. In short, if gorillas were bipeds instead of quadrupeds, their average greatest sole pad thickness would perhaps be about 6–7.5 cm. I also assume that the cushioning efficiency of the sole pad in walking modern hominids is at or near the biomechanical optimum for a bipedal mammal.

If the ankle of the Elk Wallow creatures were set farther forward on the foot than is the case in modern hominids (Krantz 1983: 60), the greatest thickness of the sole pad might occur closer to the front of the foot, and not at the heel, as is the case in gorillas and hominids. The exact position of this maximum thickness would depend on the manner in which the foot contacts the substrate—that is, whether the heel or the forward part of the foot contacts the ground first (or perhaps the entire sole contacts the ground at once). It is possible, therefore, that Krantz has greatly underestimated the thickness of the pad at the middle of the sole, perhaps close to where its greatest thickness would occur. If the average thickness of the sole pad is about 2–2.5

cm in the heel region of modern bipedal hominids with a mean weight of 60 kg (Eisenberg 1981), then it is not unreasonable to reconstruct the pad on the sole of the Elk Wallow creatures as being between 10–15 cm at its greatest thickness, if these creatures are bipedal and have a weight of 400 kg, as Krantz estimates. I am simply multiplying the thickness of the hominid sole pad by six to achieve a similar kind of cushioning efficiency in an unknown bipedal mammal whose body weight may be six times greater than that of hominids. Again, this reconstruction assumes that the cushioning efficiency of the sole pad in modern hominids is at or near the biomechanical optimum for bipedal mammals. Details imprinted on the side of the Elk Wallow prints and the edges of the footprint indentations themselves indicate that an extremely flexible sole pad is present (Krantz 1983: 64–65).

A question now presents itself. If the Elk Wallow sole pad should have a thickness of 10–15 cm at its greatest depth, and should be extremely flexible, then what is the likelihood of the prints themselves preserving an extremely detailed record of anatomy and locomotion? Would not the collapse of the sidewalls of the print impressions as the foot is withdrawn obscure fine details? The print impressions would be wider at the bottom than the top, and would be subject to such collapse. Some record of wider span at the bottom seems to be preserved in the Elk Wallow prints. Slight movements of the flexible tissue of the sole would tend to erase details, even if collapse of soil at the edges of the prints did not occur. It might be that, as body weight causes compression of the sole pad, the foot would expand and extend laterally. Analysis of gait would be incomplete or inaccurate if it did not allow for these lateral shifts, which would significantly broaden the print. Very detailed analysis of locomotion by examination of footprints to yield evidence of the sequence of weight transfer (Napier 1973), and taxonomic assignation by fine examination of dermatoglyphics might therefore be subject to a certain margin of error. Only under exceptional conditions would the substrate be able to preserve an accurate picture of the living foot. The fine-grained loess soil at the Elk Wallow site may represent such conditions, but this is not the case for most areas in which prints of creatures like those presumably responsible for these prints have been collected and studied.

Another question concerns the depth of the prints themselves. If body weight is supported by such a sole pad, the large surface area of the sole would spread the weight over a relatively wide area, so that only faint tracks would be left on hard ground. In the elephant, for example, the cushioning sole pad is so thick that the foot has an externally plantigrade appearance, although the foot skeleton is held in a semidigitigrade position. That is, the elephant's heel appears externally to be touching the ground, but the animal is actually walking with the heel portion of its foot skeleton raised. The large, thick sole pad allows the animal to traverse extremely rough terrain and move silently, but the sole pad also spreads the great body weight so effi-

ciently that individual tracks, although they cover a large area, are fainter than one might assume from the known body weight. Sikes (1971: Plate 8) shows the prints of one African elephant in firm, sandy soil. Although the print of one foot overlies the other print, pressing the soil down twice, the impressions are rather shallow. If a large sole pad spreads even massive body weight so efficiently, why do the Elk Wallow prints appear so deep? It is unlikely that the body weight exceeds the estimated 400 kg, and the sediment does not seem to have been very soft. It is possible that the sole pad of the Elk Wallow creatures is not as efficient as that of the elephant at distributing body weight over a large surface, but some type of sole pad does seem to have been present, and the estimate of its greatest depth is based on the weight inferred by Krantz (1983).

I am reasonably certain that the Elk Wallow prints are authentic. However, I do have reservations about the prints recording a precise and accurate picture of details of weight transfer. While certain portions of the casts show remarkably fine structures, Krantz (1983) is forced to select small areas from the casts to discuss dermatoglyphic evidence. This would appear to indicate that the flexible sole tissue is erasing detail. I am also puzzled by the depth of the prints. Perhaps experimentation with an artificial sole pad which could mimic the texture of the thick sole pad tissue in such mammals as camelids and elephants might resolve how much known detail can be reliably transferred from such a device onto loess soil, and thence onto plaster, and whether the dynamics of locomotion can be inferred from print and cast. The question of the depth of print impressions in relation to body weight might also be examined, if the device were capable of dissipating load like living tissue.

If the question of the depth of the impressions can be answered, the likelihood that the Elk Wallow creatures possess a large sole pad could lead to new insights into the anatomy and locomotion of such unknown animals. One interesting possibility is that the Elk Wallow creatures, like elephants, might have a semi-digitigrade foot skeleton, although the large cushioning sole pad would give the external foot a plantigrade appearance. This might account for the great amount of weight apparently carried on the forepart of the foot. Rotational movement at the ankle joint would be lost, however, so that envisioning an enlarged version of a hominid subtalar joint, for example, would be incorrect. Although Krantz (1977) has argued that the digits of the Sasquatch foot are short for biomechanical reasons, the short visible part of the digits might be arrayed at the forward edge of the sole pad, and their true length be obscured by the structure of the pad. The hindlimb anatomy and gait of the Elk Wallow creatures may be unique among mammals, and not easily inferred from comparison with living mammals of a single order, even if the dermatoglyphic evidence points to the primates.



Finally, as Napier (1973) has stressed, *trackways* of animals, and not single isolated prints, are necessary for detailed analysis of gait. Future researchers should be prepared to make casts of partial trackways—obviously not an easy task.

#### BODY SIZE AND LOCOMOTION

Sasquatch foot anatomy has been examined in detail by Krantz (1977), who argues that, in comparison with living hominids, these creatures have a lengthened heel and a shortened forefoot. This leads to a reconstruction in which the ankle joint is set farther forward on the foot, a point which is also made in Krantz (1983). The power arm of the foot is thereby lengthened and the load arm is shortened. This reconstruction is derived from Krantz's belief that changing the length of the lever arms in the foot is the simplest evolutionary solution to the problem posed by large body size in bipeds. Large body size creates a problem if body weight increases cubically, while skeletal dimensions increase areally. Hence, Krantz proposes that allometric or size-related considerations affect body design in bipeds which are significantly larger than modern hominids.

Recent research on the allometry of mammalian limb bones, however, demonstrates that the lengths and diameters of limb bones in a series of species stretching from shrew to elephant-size scale close to geometric similarity (Alexander *et al.* 1979a). Obviously, there are tremendous differences in adult body mass, but the linear dimensions of the limb bones in the series are geometrically similar. For example, the elephant femur and humerus are not very much wider relative to their length than in much smaller animals. This surprising conclusion—which upsets statements on the allometric relationship between limb bone dimensions and body weight going back to the time of Galileo—also holds true within the primates, because six primate species (including modern hominids) are included in the mammalian survey (Alexander *et al.* 1979a). This research implies that the limb bones of a biped much larger than a modern hominid need not be strikingly different in form in order to support a much greater body weight.

Stresses on limb bones of moving animals versus stationary animals have also been examined (Alexander 1977, Alexander *et al.* 1979b). Because movement causes several times more stress on the limb bones than does standing still, the increase of limb bone diameter in step with length increase becomes even more surprising. How do large animals exist in spite of what appears to be a biomechanical paradox? The answer lies in gait differences. Movement patterns of different sized mammals demonstrate that the large mammals move in such a way that their limbs remain in contact with the ground for a longer time, which decreases stress in the limb bones. The reduction of relative stresses in these bones compensates for greater body weight. The maximum stresses acting on the limbs of large animals are

comparable to those of smaller animals, so that the bones of the larger species do not need to be much more robust (Alexander 1977, Alexander *et al.* 1979b).

Two conclusions can be drawn from this research. The first is that unknown bipedal animals much larger than modern hominids, but hominid-like in position of the trunk and use of a striding gait in locomotion, are not an *a priori* impossibility. The second conclusion is that, because large animals compensate for heavy bodies mainly through gait pattern, differences between large, unknown bipeds and hominids might lie in the amount and length of time of foot contact with the ground, rather than in gross differences in skeletal structure.

#### DERMATOGLYPHICS AND THE RECONSTRUCTION OF BEHAVIOR

Dermatoglyphic evidence from the Elk Wallow prints is necessarily confined at present to description and taxonomic assignation. Nevertheless, it is possible that, if such evidence were to become more abundant in the future, details of behavior might be reconstructed from dermatoglyphic evidence. Primate species can be used to illustrate this contention.

There is a substantial body of data on the volar skin and dermatoglyphics of primates. Cartmill (1974, 1979) demonstrated that volar skin was subject to selection pressure involving allometry, with volar pad coalescence occurring in larger primates as an adaptation to increase its frictional characteristics. Biegert (1963) advocated the use of primate dermatoglyphics and volar pad structure as taxonomic traits, and in the reconstruction of phylogeny, but Meier (1980) reported evidence that dermatoglyphic pattern intensity in non-human primates may be more related to function than it is to taxonomy or phylogeny. The use of dermatoglyphics as indicators of biological distance in humans has equivocal results which do not necessarily coincide with biochemical or anthropometric variation (Meier 1980). This may not be the case in non-human primate species.

Dermatoglyphics appear to be useful in identifying individual animals, demonstrating group cohesiveness, and allowing analysis of introgressive hybridization in an Ethiopian site with troops of both olive and hamadryas baboons, and a troop of hybrids of these two species (Jolly and Peterson 1984). Analysis of dermatoglyphics in mantled howler monkeys on Barro Colorado Island, Panama, allowed inferences to be made about the evolutionary history of these monkeys on the island (Froehlich and Thorington 1982). Inferences about the colonization of one of the peninsulas by two different howler troops, and the differentiation of neighboring troops from a common troop are based on dermatoglyphic analysis. Thus, dermatoglyphic evidence can be used not simply for taxonomic or phylogenetic purposes, but for social and historical reconstruction as well. If the Elk Wallow prints and associated dermatoglyphics hold up under detailed scrutiny, and if many

additional prints with dermatoglyphics are found, the possibility exists that the genetic relatedness of individuals can be assessed, along with the possibility of differentiating variation at a group level, allowing inferences about the existence of social groups, gene flow, and the process of dispersal of groups or individuals within a given habitat unit.

#### CONCLUSION

There is a reasonable probability that the Elk Wallow prints were made by an unknown animal species. I have examined the presence of a large sole pad in some detail, and have reconstructed a sole pad whose greatest depth would approximate 10–15 cm. Two problems still concern me: 1) the degree to which the prints preserve undistorted information about dermatoglyphic pattern and locomotion, and 2) the depth of the print impressions, in spite of the presence of a sole pad which should efficiently distribute body weight.

I have also discussed the interaction of body weight and locomotion, and have indicated that, because large animals compensate for great weight mainly through gait pattern, gross differences in skeletal structure between large unknown bipeds and modern hominids are not necessarily to be expected. A larger biped would, however, need a greater length of time of foot contact with the ground. Furthermore, unknown bipedal animals much larger than modern hominids, but hominid-like in trunk position and use of a striding gait, are not intrinsically impossible.

If dermatoglyphic evidence similar to that from the Elk Wallow prints were to become more abundant, behavioral reconstruction could be attempted, given the lines of behavioral research that have opened recently from dermatoglyphic research in primates.

Finally, I question Krantz's identification of the Elk Wallow prints as "hominid," by virtue of the marked adduction of digit I (Krantz 1983: 53). Living primates show many variations in extremity structure. For example, New World ateline and Old World colobine monkeys independently reduced digit I in the hand, sometimes to a remarkable degree. This would be an example of the parallel evolution of a trait in two different infraorders of living primates. Conversely, it is also possible for members of a single family to show remarkable divergence in extremity structure. Thus, the African great apes develop knuckle-walking specializations of the hands while the orang-utan does not, instead lengthening the entire hand and digits II–V and reducing digit I. The foot of the orang-utan is similarly lengthened, with concomitant elongation of digits II–V, and reduction of digit I, sometimes to a vestigial state. Given the great evolutionary potential for variation in extremity structure documented in living primates, it may be problematic to identify the Elk Wallow creatures as hominids by virtue of a markedly adducted hallux. In any case, Krantz also lists a number of traits which

completely differentiate the Elk Wallow prints from those of modern or extinct hominids, such as the anterior placement of the ankle and extremely short digits. Although reported sightings of these animals emphasize similarities to hominids, and dermatoglyphic evidence points to the primate order, analysis of the prints demonstrates the existence of foot structure and locomotion unlike anything known among mammals. The prints are not simply enlarged and broadened versions of hominid prints. Therefore, the makers of the prints do not have the anatomy, body proportions, and locomotion of living hominids.

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## Research Reports

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### ESTIMATING THE PROBABILITY OF NON-DETECTION OF LOW DENSITY POPULATIONS

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**ABSTRACT:** A method for estimating the probability of presence or absence of cryptic species was developed using the binomial distribution. For a given probability of detection ( $p$ ) at a known population density during a unit effort of searching, the probability of non-detection during a specified number of search efforts can be estimated.

#### INTRODUCTION

Confirming the presence of elusive, secretive, or rare species in a large geographic area can be a frustrating task. Providing proof that the species is absent from an area may be impossible, but some form of an opinion statement concerning the presence of a population is often required as part of environmental assessments or for use in developing resource management schemes. When the species in question is an endangered one, the confirmation of its presence or absence may be required by law. If such a statement is based on rigorous, scientific study, with stated probabilities of error, it is more likely to be viewed as a reliable estimate rather than a subjective opinion.

This paper presents a conceptual framework for estimating the probability of presence or absence of species that are, by their very nature, difficult to observe. We will develop estimates of probabilities of non-detection. That is, given a specified density of the desired population, what would be the probability of no sightings for a specified level of searching?

## METHOD

The probability of non-detection for a species at a specified population density can be estimated for any degree of searching, if the probability of finding a conclusive sign of the species during a single search effort is known for the species in question at the suspected density in the habitat being searched. If a search effort can be standardized, i.e., one unit of search effort equals searching for tracks along one mile of sandy road after a rain, or listening for vocalizations during the first hour after sunrise, etc., then the expected number of successful searches,  $x$ , for an effort of  $n$  searches can be described by the binomial distribution.

When this distribution is used, two assumptions must be met. The probability of detecting conclusive sign during one unit of search effort must be constant for all search efforts, and the probability of success during any one search effort must be independent of the success of other efforts. Given these assumptions, the distribution is characterized as:

$$f(x) = \frac{n!}{x!(n-x)!} p^x q^{n-x}, \quad x = 0, 1, 2 \dots n \quad (1)$$

where  $f(x)$  = probability of  $x$  successful searches efforts during  $n$  attempts

$p$  = the probability of success for any one search effort, and

$q = 1 - p$ .

Estimates of  $p$  are dependent on the abundance, distribution, persistence, and distinctiveness of the conclusive signs, and the ability of the observer to notice them. Each of these factors is influenced by habitat, season, weather, human disturbance, and the sex and age structure of the population, as well as population density. Hence, estimates of  $p$  cannot be determined for a species in general, but must be made for each specific species-environment to be studied.

Data on resightings of marked white-tailed deer (*Odocoileus virginianus*) fawns were used to examine the feasibility of the binomial distribution model. The study area was an 826-ha enclosure on the Radford Army Ammunition Plant, Dublin, Virginia. A 2.4-m high cyclone fence topped with barbed wire formed the enclosure. The habitat was rolling grassland with small isolated stands of mature hardwoods and scattered red cedars (*Juniperus virginianus*). Approximately 80 ha of the enclosure have been planted with shortleaf pine (*Pinus echinata*), but few deer use these stands for cover. The area is traversed by an extensive road system that affords excellent observation opportunities throughout the entire enclosure.

Fawns were captured, marked with a unique set of colored ear streamers, and released during the Spring of 1969 as reported by Downing and McGinnes

(1969). A 30-km route was driven twice daily and sightings were recorded for each of the individually marked fawns. Searches were discontinued after three weeks, at which time the fawns were old enough to begin travelling with their mothers, and lost much of their cryptic nature. Detection of conclusive sign was defined as sighting a fawn during the 30-km drive. The 30-km route was the search effort unit. Seventy-three of the 81 fawns were known through later observations to have survived the three-week observation period, and those served as our search subjects. The search route was traversed 24 times during the observation period. Because deer were individually tagged, we had the equivalent of 1,752 search efforts for individual deer. A total of 73 sightings occurred during the search. Thus, the probability of sighting an individual fawn was:

$$p = \frac{73 \text{ sightings}}{1,752 \text{ efforts}} = 0.0417$$

## RESULTS

By substituting  $p = 0.0417$  and  $n = 24$  into equation 1, we calculated the expected distribution of sighting frequencies ( $E_f$ ) for an individual fawn. Our observed frequencies were not statistically different from expectations given a binomial distribution (Table 1). The probability of not detecting a specific fawn during  $n$  search efforts is calculated as:

$$f(x = 0) = q^n \quad (2)$$

For  $p = 0.0417$  and  $n = 24$ ,  $f(x = 0) = (1 - 0.0417)^{24} = 0.36$ . The number of search efforts required for a specified  $f(x = 0)$  is calculated as:

$$n_0 = \frac{\ln[f(x = 0)]}{\ln q} \quad (3)$$

For  $f(x = 0) = 0.05$ :

$$n_0 = \frac{-2.9957}{\ln q} \quad (4)$$

For  $p = 0.0417$  and  $f(x = 0) = 0.05$ ,  $n = 71$ . Searching for fawns with an assumed density of 1/826 ha and  $p = 0.0417$  would require 71 unsuccessful search efforts before there was less than a 5% chance of error in concluding that the animal was truly absent.

The relationship between the number of required search efforts and specified levels of  $f(x = 0)$  can be calculated for an array of  $p$  values (Fig. 1).



TABLE 1.—Observed and expected frequencies assuming a binomial distribution of sightings of individual marked fawns during 24 search efforts on the Radford Army Ammunition Plant, Dublin, Virginia (1969).

No. of sightings after capture (x)	Observed frequency (O <sub>x</sub> )	Expected frequency (E <sub>x</sub> )
0	26	26.3
1	29	26.3
2	9	13.7
≥ 3	9	5.7

$$X^2 = \sum_{x=0}^3 \frac{(O_x - E_x)^2}{E_x} = 3.85^*$$

\* Not significant at  $\alpha = 0.05$ , 3 d.f. Frequencies for 3, 4, 5 and 6 sightings were combined so that O<sub>x</sub> and E<sub>x</sub> ≥ 5.

### DISCUSSION

Anyone planning to search for cryptic animals should attempt to determine the probability of observing conclusive sign, so that searches can be designed to have a statistically significant result. Nothing can be concluded after 10 days of unsuccessfully searching an area, for example, if 20 days are required to state with 95% confidence that the animal being sought is not there. In fact, an insufficient effort may do more harm than good, because persons who are not familiar with statistical probabilities may conclude that the animal is not present after 10 unsuccessful search attempts, when such a conclusion may have a high probability of error. In order to obtain credible appraisals of presence or absence of cryptic species, the probability of detection should be known. Rigorous, scientific, quantified search efforts reported with their associated probabilities of error are far more desirable than popular sentiment and hearsay evidence. In situations where estimates of detection probabilities are costly or difficult to obtain, a range of likely detection probabilities can be used to judge the adequacy of searching effort.

The cougar study reported by Downing (1984) is a case in point. Downing recognized the need to measure the probability of observing cougar sign before beginning a systematic search of the Appalachian Mountains, in the southeastern United States. However, he also recognized that cougar tracks are indistinct in these mountains, due to the hard soil conditions. Measures of the probability of observing cougar sign are possible to obtain in Florida, the western United States, and South America; but these might not be applicable to Appalachian conditions. Reports of cougar sightings were so frequent that he enlisted the aid, through publicity and training, of hundreds of cooperators to find signs of a few cougars. These cougars could be intensively studied to estimate the probability of observing sign. Unfortunately, no cougars were found in the three years allocated to this broad-brush approach, and funds were not available to go to a comparable area known to contain cougars. Another possibility would have been to record the frequency

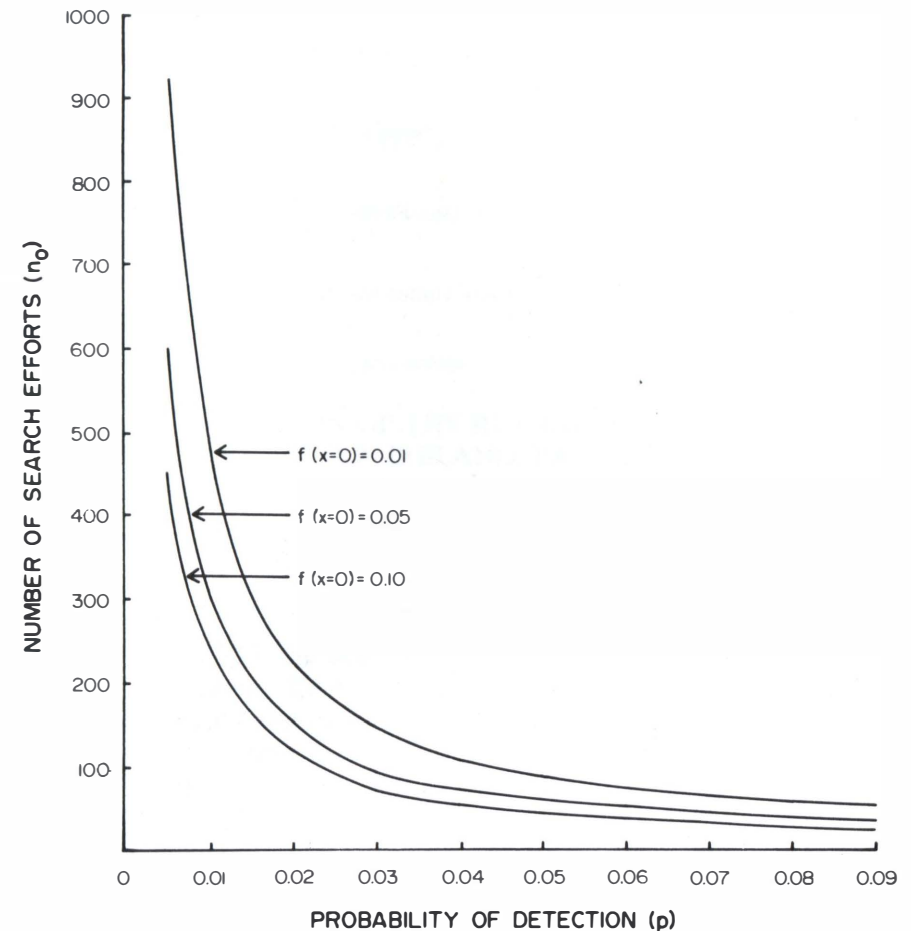


FIG. 1.—Number of search efforts ( $n_0$ ) required at  $f(x=0) = 0.01, 0.05, 0.10$  at values of  $0.005 \leq p \leq 0.09$ .

of observing the tracks of bobcats (*Lynx rufus*), the nearest local relative of the cougar, and make some appropriate behavioral adjustments. Such adjustments would have included consideration of how far each species travels each night, and differences in where they travel that might affect track observability. Before Downing could determine the probability of observing sign, and hence attach some estimate of reliability to his findings, the project was closed because of the lack of evidence from the three-year broad-brush study.

We recommend using a range of  $p$  values which will likely encompass the "true" value of  $p$  for the specific population-environment-technique situa-

tion, and then calculate  $f(x)$  for a single sighting (or some other number of sightings) across a range of  $n$  values. This matrix of  $f(x)$  values can serve as an information base for planning and feasibility studies.

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## Field Reports

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### IDENTIFICATION OF THE RI THROUGH FURTHER FIELDWORK IN NEW IRELAND, PAPUA NEW GUINEA

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#### INTRODUCTION

In February, 1985, the author and twelve others from the Ecosophical Research Association (ERA) conducted fieldwork in Papua New Guinea, principally along the east coast of southern and central New Ireland Province. The purpose of the fieldwork was to continue investigations into reports of an unidentified marine animal variously called *ri* (in Barok), *ilkai* (in Susurunga), or *pishmeri* (in New Guinea Pidgin) by the native peoples. Previous reports and the results of earlier fieldwork had proved inconclusive (see Roy Wagner, 1982, *The Ri—Unidentified Aquatic Animals of New Ireland, Papua New Guinea*, *Cryptozoology*, Vol. 1: 33-39; Roy Wagner, J. Richard Greenwell, Gale J. Raymond, and Kurt Von Nieda, 1983, *Further Investigations into the Biological and Cultural Affinities of the Ri*, *Cryptozoology*, Vol. 2: 113-125).

The work of the expedition undertaken by Wagner *et al.* in 1983 produced visual observations that were consistent with information provided by Barok and Susurunga natives, but did not correspond unambiguously with the known behaviors of familiar marine mammals, such as cetaceans or sireneans. Photographic results of the previous expedition were also inconclusive. There was, however, enough hard evidence of an unidentified mammal that they considered further investigation, and even another expedition, to be warranted, and they recommended that such a new expedition be equipped with better photographic gear and a powered boat.

It was decided by ERA that a powered boat was indispensable, as it would provide a direct access by sea to the animal's habitat, as well as serve as a



mobile base of operations. To utilize an adequate vessel, however, it was necessary to sail it from Port Moresby, capital of Papua New Guinea, to the site of the investigation, a distance of over 1,000 miles. The boat chartered was especially equipped for diving operations. The TSMV *Reef Explorer* is a 65-foot, twin-screw motor vessel with radar, side-scan sonar, satellite navigation, ship-to-shore radio telephone, and an air station for filling scuba tanks. In addition to standard photographic equipment, it was decided to include a video crew with adequate surface and underwater video equipment.

After discussing the advisability of trying to capture an animal, it was decided to rely solely on photographic and video evidence. If we were successful in establishing the existence of a new species of marine mammal, we knew it would be vital to work closely with the Papua New Guinea government. Contact was established with the Provincial Premier, Mr. Robert Seeto, who expressed interest in our findings and requested a written report on our activities.

#### NARRATIVE DESCRIPTION

Starting with our departure from Port Moresby and during our journey along the southern coast of Papua New Guinea to Milne Bay, at the eastern tip of the New Guinea land mass, we encountered numerous accounts of "mermaid-like" creatures, which, the people insisted, were a combination of a human (in most accounts female) head and torso with a "fish-like" tail. Reports ranged from very realistic sounding accounts to stories containing obvious elements of folklore. One individual, for instance, held that throwing garbage overboard would anger the *pishmeri*. Curiously, no stories of merfolk were encountered during a stop in the Trobriand Islands, although the Trobrianders are well known as sea-faring people. On February 10, 1985, the *Reef Explorer* arrived off Cape St. George, at the southern tip of New Ireland island. The boat anchored in Louisa Bay at about 5:30 a.m., and a watch was set to look for any evidence of the Ri. No sightings were made in Louisa Bay, and it was decided to move on towards more promising sites, such as where earlier observations by the Wagner team had been made, in Nokon Bay. En route, several members went ashore for a short time at Muliama Harbor, and talked with the local people about Ri stories. The natives were quite familiar with the animal, and called it by its Pidgin name, *pishmeri*.

The *Reef Explorer* arrived at Nokon and dropped anchor in the bay at 1:45 p.m. Almost immediately, at 2:00 p.m., two expedition members, Morning Glory Zell and Rich Bergero, sighted the flukes of an animal above the water on the south side of Nokon Bay. At first, they looked like the flukes of a small whale, but subsequent observations caused that impression to be modified. Observations were made from 2:00 p.m. until about 5:30 p.m., from both the surface and, once, from underwater. Surface observations revealed flukes or a rolling back, often with a head visible. The animal appeared to surface in periods ranging from 8 to about 11 minutes, which

is consistent with the observations by the Wagner team. In addition, another distinctly smaller individual was sighted. When the animal's back broke the surface, it had the appearance of a sharp, quick roll, with no visible dorsal fin.

Attempts to photograph this activity with still cameras were disappointing. It was possible, however, to get some excellent results from the beach using the video equipment. Our observations and video footage confirm the observations by Wagner and Greenwell of extreme vertical flexure, and flukes appearing above the surface during a dive.

The display of the flukes was quite spectacular. At times, the flukes would appear above the water in a graceful, sweeping motion until the tail was completely vertical. The ends of the flukes would flop over or droop slightly as the tail slid beneath the surface. The color of the tail appeared brown, but somewhat lighter at the edge. Most surface sightings were made from the boat, which was anchored in the approximate center of Nokon Bay. The animals were always observed on the south side of the boat, and to the left of the small point at the end of the bay (Fig. 1).

About that time, a native called Tom Omar approached the *Reef Explorer* in a rowboat. When asked about the Ilkai, he pointed to a tail that was just breaking surface and exclaimed: "Ilkai, Ilkai, em i stap!" ("There it is"). He then went on to describe the female as having a woman's face, hair, hands, and breasts, saying that there was a family living in the bay: a male, a female, and a child. During the conversation, the animal was displaying the rolling back and tail activity described above.

The first underwater observation was made by the Captain of the *Reef Explorer*, Kerry Piesch. At about 3:30 p.m., Captain Piesch set out from the boat with fins and snorkel and a small underwater camera. He was accompanied by expedition member Bergero. Shortly thereafter, Capt. Piesch signalled that he had observed and photographed the animal underwater. On returning to the *Reef Explorer*, Capt. Piesch gave a description of what he had seen, which is summarized below:

Length was approximately 5 to 5½ feet, and the color appeared a greenish-gray underwater. There was a distinct head that joined to the body with no discernible neck. The forelimbs were short and paddle-shaped, but the face could not be seen clearly from the observer's position. The hindquarters tapered off and rounded in a very streamlined shape, ending in the tail described above. The forelimbs showed no evidence of hands or fingers, but ended in what appeared to be three lobes. The animal, which resembled a dugong, moved easily, with graceful undulations of its tail, and Capt. Piesch was able to take three photographs before he had to come up for air. He noted that, when the animal saw him, it moved away, but did not seem unduly alarmed.

The morning of February 12 produced still more observations. Expedition members Munro, Hill, Wood, and Bergero set out in the dinghy with video

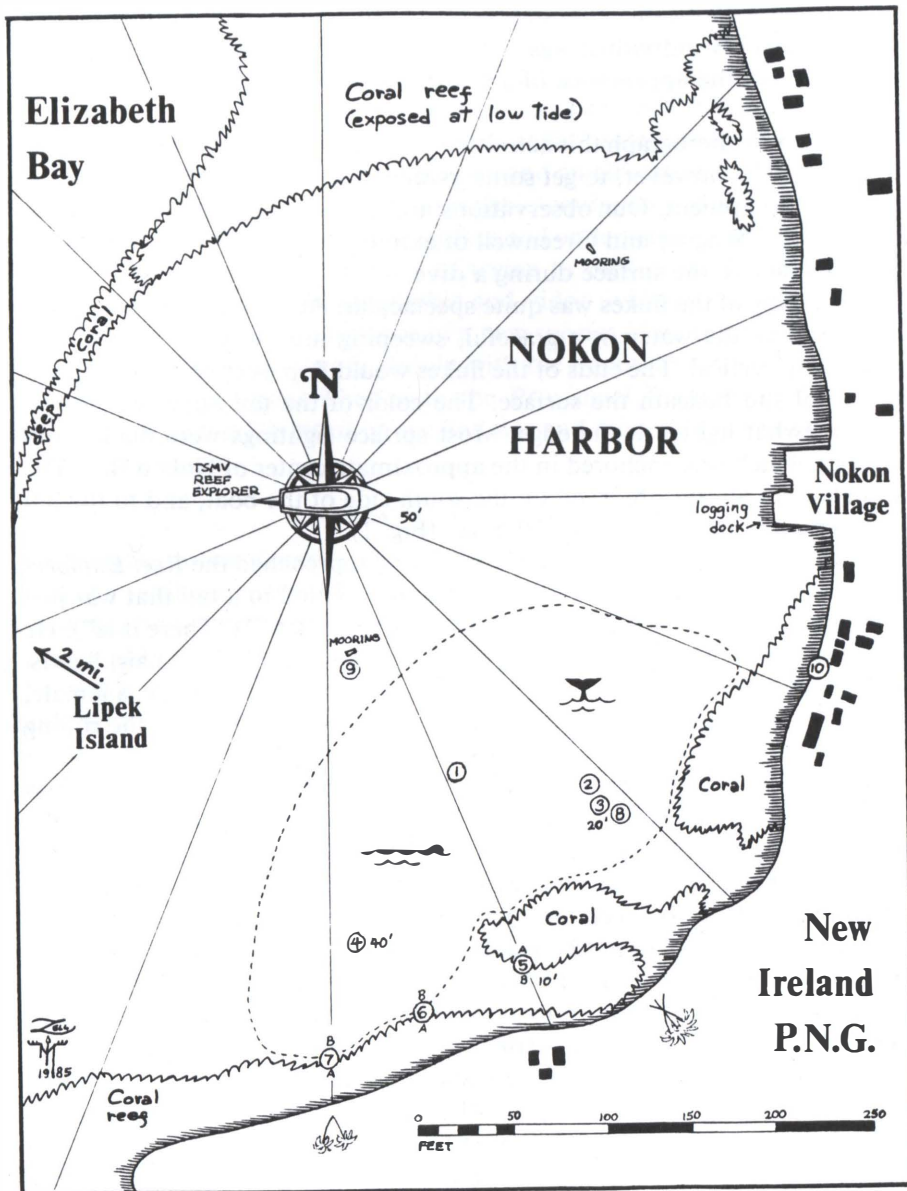


FIG. 1.—Map of Nokon Bay by Otter Zell. Dotted line represents general area of observed surface activity. Numbers 1, 2, 3, and 4 represent sites of actual recorded surface observations. Numbers 2 and 3 also represent sites of underwater photography. Number 4 represents the site where feeding furrows in the bottom sand were found. Numbers 5, 6, 7, and 8 represent sites where vegetation was collected.



FIG. 2.—Dugong photographed underwater during feeding activity in Nokon Bay. Photograph was taken shortly after observation of surface activity. (Photo: Rich Bergero.)

gear to set up on the south beach of the bay. While stationed there, they were able to record several surfacings of the type described above. A light rainstorm came up at this time, and Bergero, who had remained in the dinghy with the first mate, started to paddle toward the camera crew on shore. He then went into the water with fins, snorkel, and camera, leaving the first mate to pick up the shore party.

Bergero and Weber, who had also gone into the water with a camera, were able to obtain underwater photographs of the animal (Fig. 2). Bergero seemed convinced that he had seen a dugong. Weber tended to agree, but they differed as to whether the tail was the same as had been observed on the surface. Both, however, noted that the animal, when observed, had been nuzzling with its snout in the sandy bottom of the bay, making a furrow as it went. It would furrow a little ahead, then back off a bit and dig into the furrow, then continue forward. On seeing two divers approaching from different directions, the animal became alarmed, and it retreated.

Since the animal appeared to be in the habit of coming into the bay to feed only in the mornings and afternoons, no further observations were made until about 3:00 p.m. that day. More surface activity began at that time, and an attempt was made by the video team to get some video footage of the animal underwater. This attempt was not successful due to the bulky nature



Type D. Ilkai Kai Kai



FIG. 3.—Field sketch by Daniel Blair of vegetation found in dugong feeding area (sandy bottom, 40–50-foot depth), and in mouth and stomach of dead dugong.

of the video equipment in the small dinghy, and unfavorable wind conditions. During the attempts to videotape the animal underwater, the rolling motion and tail fluke activity was observed from the *Reef Explorer*, but the

crew was never able to be in the right place at the right time to obtain photographs. After this attempt, it was decided to pursue diving activities elsewhere on the following day, in order not to unduly harass the animal.

On the morning of February 14, divers with scuba gear surveyed the bottom of the bay in the area where the animal had been seen feeding, in an attempt to see what kind of vegetation or shellfish it was eating. Traces of furrowed marks were seen and photographed on the sandy bottom, along with a small bottom plant. Samples of this plant were collected and photographed for later identification. The association of this plant (Fig. 3) with the furrowed marks indicates that it was the animal's primary food source in the bay. No evidence of sufficient shellfish for the diet of such an animal was found in the feeding area. A second attempt to obtain video footage of the animal was also unsuccessful.

The morning of February 15 brought an abrupt end to activities at Nokon. Early in the morning, some villagers were observed pulling a large animal out of the water onto the beach. When several expedition members got to the beach from the *Reef Explorer*, it was clear that the animal was an adult female dugong. She had apparently been killed by a single wound in the right dorsal portion of the body, slightly behind the right flipper. Subsequent internal inspection revealed that she had been shot by a high-powered rifle. While the bullet was not recovered, the chest cavity showed massive damage, such as can only be caused by a soft-nosed or hollow point rifle bullet. Further inspection also disclosed the plant observed in the feeding area in the animal's stomach and mouth. Exactly who had killed the animal or just when it had happened could not be determined.

Otter Zell retained the skull of the animal, hoping to take it back to the U.S. for positive identification. Permission to export the skull was, however, refused by officials in Madang.

#### RESULTS

The ERA expedition has had two major results. First, the Ri/Ilkai has been positively identified as the Indo-Pacific dugong, *Dugong dugon*. The combination of visual sightings, both above the surface and underwater, along with photographic evidence and the unfortunate death of an animal at about the same time and place, make this conclusion inescapable. In fact, it is the consensus of expedition members that this same conclusion would have been reached if the death of the animal had not occurred.

Second, several new facts have been learned about dugong behavior, and these are related to the reasons that caused the members of the Wagner expedition to reject the dugong hypothesis, as proposed previously in this journal (see James G. Mead, 1983, Ri or Dugong?, *Cryptozoology*, Vol. 2: 161–162; John R. Sibert, 1984, Semantics versus Cryptozoology, *Crypto-*

zoology, Vol. 3: 144–145; John-Erik Beckjord, 1984, Ri Evidence Lacking, *Cryptozoology*, Vol. 3: 154–155). Specifically, these are: 1) the average submergence time observed by our expedition; 2) the observation of extreme vertical flexure of the animal's body at the surface; and 3) the display of flukes—all of which confirm the observations of Wagner and Greenwell (see Wagner *et al.*, 1983, above).

The observed average dive times of about 10 minutes conflict strongly with the approximately one minute dive times reported previously in the scientific literature (see Paul K. Anderson and Alastair Birtles, 1978, Behavior and Ecology of the Dugong *Dugong dugon* [Sirenia]: Observations in Shoalwater and Cleveland Bays, Queensland, *Australian Wildlife Research*, Vol. 5: 1–23). It should be noted that Anderson and Birtles' observations were made in water considerably shallower than that at Nokon. An inspection of the animal's bottom feeding area in Nokon Bay by the author and Robert Weber using scuba gear showed a depth of between 40 and 50 feet. Thus, the extreme vertical flexure observed in Nokon Bay may well be related to an additional submergence mode when the dugong is diving in deeper water, and it is then, it seems, that the dugong stays submerged far longer than that recorded previously in shallower water. These possibilities were hinted at by the Wagner team members themselves in the form of a personal communication from Anderson (see Wagner *et al.*, 1983, above).

Although the natives of Nokon traditionally never harm the dugongs in their area, there are plenty of tales of other people who hunt and eat the Ri, and even sell its meat in the markets. Whether these tales are part of a ruse to circumvent Papua New Guinea law protecting the dugong remains uncertain. There is strong indication, however, that, since the Ilkai—and thus the Ri—has been identified as the dugong, any hunting or dealing with the meat of such supposed “merfolk”-type creatures is, in reality, dealing with dugongs, and should be prevented, particularly since the dugong is listed as endangered. We have passed these observations on to the Provincial Government of New Ireland.

#### FUTURE PLANS

As a cryptozoological problem, the question of the Ri/Ilkai appears to be solved. Therefore, we have no plans for future fieldwork. We are pleased to have been able to solve such a problem, and hope that the results of our expedition may serve as an example for future cryptozoological field efforts. We also wish to express the hope that the dedicated, open-minded efforts of earlier researchers in the question of the Ri as a cryptozoological problem, which culminated in the results of our own work, will help enhance the acceptance of cryptozoology as a valid scientific pursuit.

## LCPI WORK AT LAKE CHAMPLAIN, 1985

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### INTRODUCTION

The possibility that the 109-mile-long Lake Champlain is the habitat of Loch Ness-like animals, dubbed Champ, continues to intrigue many individuals and groups, the Lake Champlain Phenomena Investigation (LCPI) included. The Champ controversy remains unresolved, but continued research and fieldwork by LCPI and others has uncovered more data. This report serves as an update on ongoing research and fieldwork (see Joseph W. Zarzynski, 1984, LCPI Work at Lake Champlain, 1984, *Cryptozoology*, Vol. 3: 80–83).

The 31 days of 1985 LCPI fieldwork was directed at: (1) a continuation of daylight surface surveillance with personnel using an array of shore-based binoculars and camera (still, movie, and video); (2) further testing and use of a night-sight scope for nocturnal surface surveillance; (3) an underwater search for Champ utilizing sonar and scuba monitoring; (4) further documentation, analysis, and publication of Champ sightings; (5) encouraging residents and visitors to the lake to carry cameras for possible photographic documentation of Champ; and (6) providing assistance to responsible individuals and groups in Champ-related projects.

### NARRATIVE DESCRIPTION

The 31 days of LCPI fieldwork at Lake Champlain was executed primarily by Mary Patram Meaney and Joseph W. Zarzynski. Assistance was also provided by: Ted Straiton (July 5, August 28, September 1, October 5–6), Jack Sullivan (August 4, 8; September 1, 8), Bruce Hallenbeck (August 5, 6), Tim Clark (July 29, 30), John Becker (October 5, 6), Ray and Salome Keller (October 5, 6), Richard Smith (July 19), and Richard Cowperthwait and Lauren Murphy (August 1).

Fieldwork consisted of: daytime surface surveillance using 35 mm cameras, a Super 8 mm camera, a long-range television video camera and recorder, telephoto lenses, and camera tripods; nocturnal surface surveillance using a hand-held night-sight scope; a sonar (Raytheon DE725C) search from a 4.5-foot-long aluminum tripod deployed in 35 feet of water; scuba monitoring used primarily to position the sonar tripod underwater; scuba diver underwater photography; and a search for the reported “skeleton remains of a whale” in the lake.



Lake surface surveillance was conducted from: Kimball Dock Pier, Vermont, Button Bay, Vermont, and other lake locations. Three boat surface surveillances were also conducted.

Five Champ-related scuba diving sessions were done (four for deployment, monitoring, and retrieval of the Raytheon sonar tripod, and one as part of an underwater reconnaissance survey). Two of these scuba dives were done by Meaney and Zarzynski, and the other three by Meaney, Zarzynski, and Sullivan.

An underwater search using a diving team with wet suits, mask, fins, and snorkels was also conducted in an attempt to locate the site of reported skeletal remains of a whale. Archival research this past summer in Albany, New York, uncovered documentation circa 1919–21 that the remains of a large, unknown animal, presumably a whale, had been discovered in Lake Champlain. Working with archivists, LCPI undertook a preliminary on-site search. Further follow-up research and fieldwork must be completed to ensure that all avenues of this investigation have been explored. This account possesses a significant measure of credibility since, in 1849, the bones of a 14-foot-long white whale (*Delphinapterus leucas*) were discovered in Charlotte, Vermont, on the shores of Lake Champlain, during work by the railroad. If there is any truth to the present 65-year-old report, then the remains could be of a white whale, or even of a Champ animal (it should be remembered that one of the candidates for Champ is the zeuglodon, a primitive whale believed extinct for many millions of years).

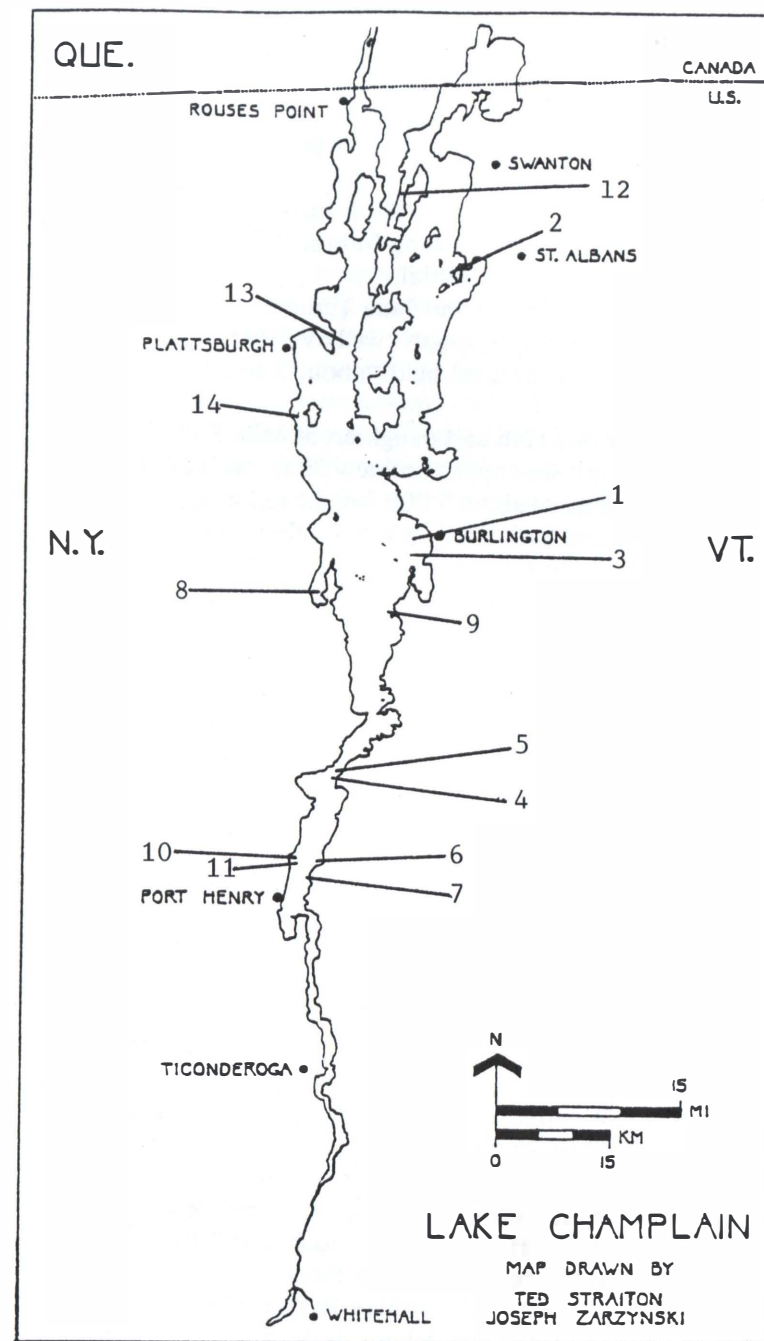
The dates of the 31-day 1985 LCPI fieldwork were: June 22–29; July 5, 19, 27–31; August 1–9, 22, 28; September 1, 8; October 4–6. Day surface surveillance using cameras and binoculars was conducted during all 31 days; nocturnal surface surveillance was used during 10 days; sonar deployment and usage during nine days; five scuba dives; boat surface surveillance during three days; and one day conducting a diver snorkel search.

#### RESULTS

LCPI fieldwork in 1985 did not result in any visual sightings of a supposed Champ animal. Intriguing targets picked up by our Raytheon DE725C sonar were analyzed by Jim Kennard, a member of LCPI and an engineer and sonar expert. He reported that the targets were from fish, not from one or several Champ animals.

Nevertheless, LCPI was able to document 14 Champ sightings reported during 1985 (Fig. 1). A short listing of these sightings appears below in chronological order:

FIG. 1.—Map of Lake Champlain, with numbers indicating the locations of the 14 eyewitness sightings logged by LCPI during 1985.



- May 6, 1985: Alice and Al Brooks; off Battery Park, Burlington, Vermont; 15–20 feet long; it “looked like a long telephone pole propelled through the water with mild wake behind it.”
- May 22, 1985: Lillian Cayo and 3 others; off Lapans Bay Pt., Vermont; 30 feet long; greenish-brown-very dark in color, with humps or ridges on its back.
- June 25, 1985: Dennis Hall; from the Radisson Hotel, Burlington, Vermont; first one large 15-foot-long hump, then a second hump which Hall believes was another animal.
- June 29, 1985: Peg McGeoch and Jane Temple; off Scotch Bonnet, south of Basin Harbor, Vermont; length “well over 30 feet”; head/neck similar to a brontosaurus, with head held “about 5 feet above surface”; body was snakelike.
- June 30, 1985: Dennis Hall and daughter; at Mile Point, near Basin Harbor, Vermont; Hall shot approximately 20 seconds of inconclusive Beta videotape at a range of about 5,000 feet; he reported he saw the animal’s head and neck, and estimated its size at about 30 feet.
- July 1, 1985: Pete Horton; in Potash Bay, Vermont; an “unidentified submerged object” with “three, rounded black humps which were proceeding smoothly parallel to the shore”; length 20–30 feet.
- July 2, 1985: Ann Koch and Rita Shaffer; off Elm Point, near Potash Bay, Vermont; a 10-foot-long segment; no head seen; Koch claimed it was “not a fish, not an eel, not a snake”; darkish in color.
- July 30, 1985: William Billado, Jr., Gerald C. Milot, and Theodore Kessler; near southern tip of Willsboro Bay, New York; at 11 p.m., from their boat, at a range of 12–15 feet; two humps exposed that totalled 3 or 4 feet, but they estimated the animal’s length at 15–20 feet.
- August 5, 1985: Jody Kompanek, husband, and son; west of Meach Cove, Vermont; “. . . it looked like a huge snake moving through the water in an up and down motion . . . . We saw the head . . .”; 25 feet long.
- August 8, 1985: Jean and Becky Joppru; in Mullen Bay, New York; 4 or 5 black humps protruding 2 or 3 feet from water; total length, 30 feet.
- August 9, 1985: Becky Joppru; just south of Mullen Bay, New York; similar to August 8, 1985 sighting, but 35 feet long.
- August 19, 1985: Wynfred Leary and daughter; from North Hero, Vermont (near Mother’s Restaurant); “4 or 5 humps, black and shiny and round like a rubber tube going about 5 miles per hour”; 25 feet in total length.
- August 20, 1985: Sally Coppersfield and 2 children; from Adams Landing, Vermont; 2 dark humps, each 3 feet long and 2 feet out of the water; the humps appeared to have fins on them.
- September 2, 1985: June and Geoffrey Barker; about 100 yards north of Salmon River, near Valcour Island, New York; Barker took two pho-

tographs of a wave phenomenon that might be from an animal, but he admits the photographs are not helpful; observed from 28-foot-long cruiser.

#### FUTURE PLANS

LCPI will continue its field operations and research at Lake Champlain using strategies similar to those which have been conducted over the past several years. We also intend to complete our work regarding the 1919–21 report of a skeleton of a whale or an unknown animal reported to be in Lake Champlain. LCPI shall likewise be working for the adoption of the “Champ Resolution” by the Vermont Senate during the 1986 Vermont Legislative Session. The resolution calls for Champ’s protection, encourages further scientific inquiry into the phenomenon, and asks people to report their Champ sightings. The resolution has already been adopted by the Vermont House of Representatives, the New York State Senate, and the New York Assembly. Since three Vermont State Senators have announced their intention to co-sponsor this resolution in the Vermont Senate, chances are good that it will pass.



## INVESTIGATIONS IN THE LAKE CHAMPLAIN BASIN, 1985

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### INTRODUCTION

In July 1985, Wind & Whalebone Media Productions continued its investigation into the matter of large unknown animals reported in Lake Champlain, said to be "Loch Ness-like animals," and popularly referred to as Champ. This season's research was a continuation of fieldwork begun last year (see Richard D. Smith, 1984, Testing an Underwater Video System at Lake Champlain, *Cryptozoology*, Vol. 3: 89-93).

The research methodology continued to be guided by considerations of the reported animals' three-dimensional, "drowned valley" habitat, with emphasis on introducing probes into sectors where contact with such animals is deemed most likely to occur. This fieldwork also continues to integrate basin soundings, survey location, and underwater video deployment, in order to yield results as definitive and repeatable as possible.

The specific intent of the 1985 work at Lake Champlain was: (1) to employ a chart recording sonar device operated from a boat, in order to obtain records of bottom contours, as well as the presence of fish and any larger aquatic animals; (2) to cross-reference the sonar chart data with navigational and bathymetric charts by taking position sightings from the boat during operations; (3) to deploy and test a newly designed underwater video system, and to make underwater hydrophone sound recordings; (4) to gather any data of use to professional ichthyologists, geologists and limnologists; (5) to gather any information on previously unrecorded sightings of Champ, and to relay this information to the Lake Champlain Phenomena Investigation (LCPI) (see Joseph W. Zarzynski, 1984, LCPI Work at Lake Champlain, 1984, *Cryptozoology*, Vol. 3: 80-83).

Wind & Whalebone fieldwork at Champlain in 1985 consisted of 24 days on site, with 12 days spent on water in data-gathering operations, with the remaining period spent on equipment preparation and repair, resupply, travel, interviews, and miscellaneous down time.

Exercises were coordinated by the author. Joining him on site and assisting in operations were Wendy Lathrop (July 12-20), Ike and Lois Blonder (July 21-26), and Gary Mangiacopra (July 21-26). Important logistical support was provided by John Andrews, Robert and Barbara Beach, the John Durant family, Dennis Hall, Phil Keller, and Ed Parsons.

Joe Zarzynski, LCPI director, visited our base at Essex, New York, on July 19 and 25. Mr. Zarzynski shared valuable information and insights, as did Mr. Blonder, who is a board member of the Academy of Applied Science

and a veteran of the Academy's Loch Ness work, and Mr. Mangiacopra. Although Wind & Whalebone's lake research is not supported by the LCPI or AAS, the author is grateful to have participated in a cooperation between individuals sharing common goals in this area of cryptozoological research. Acknowledgement is made of the additional support of Robert Durant and Margaret Light of this season's work.

### NARRATIVE DESCRIPTION

Basin sounding work was carried out utilizing an Apelco MR-201C indicating and recording sonar, manufactured by the Raytheon Corporation. Sonar has been used previously in efforts to ascertain the presence of large, unknown animals in Lake Champlain, as well as Loch Morar, and, most notably, Loch Ness.

Sonar soundings were carried out this season in the Champlain basin at the following locations: off Essex, New York (July 7 and 16); Whallon Bay vicinity (July 8, 18, 22 and 23); at the mouth of Willsboro Bay (July 10); north of Willsboro Point (July 17); in the channel west of Valcour Island (July 11); and off Scotch Bonnet, south of Basin Harbour, Vermont (July 13, 14 and 24). Hydrophone recordings were made during the last three sessions in Whallon Bay.

The Scotch Bonnet work centered around the site of a fairly recent report of a Champ animal, to which the author was first alerted by Joe Zarzynski. Peg McGeoch and Jane Temple claimed to have observed this animal at around 4:00 p.m. on June 29, 1985, from the porch of a vacation cottage which they were housesitting, and which overlooked the lake.

The July 24 session involved having the two eyewitnesses separately direct the chart recorder boat (via two-way walkie-talkie radio communication) over the path each recalled the animal as having taken, and at the same speed. Three passes were made under each woman's direction, two in the original heading, and one reversing. In addition, the women were each asked on one pass to note where they remembered seeing the animal surface and submerge. These locations were marked on the chart at the moment they were given. To the author's knowledge, an examination of an eyewitness account of a large, unknown aquatic animal has not previously been made in this manner.

During all of the season's boat operations, sightings on prominent shore-line topographical features were periodically taken with a sighting compass, particularly when chart recordings of interest were being noted. The position of these surface features was measured in degrees off magnetic north, for later comparison with navigational charts, and also to allow maintenance of a particular course, or relocation of a particular position during actual operations.

Small boats (14 feet and under) were used. Although some were powered

by outboard gasoline motors, during actual sonar use the boats were, with only a few exceptions, rowed, propelled by an electric trawling motor, or allowed to drift. Noise was thus reduced during sonar operations in order to minimize possible avoidance behavior by the target animals.

As much as weather conditions, boat availability, and other factors would allow, an effort was made to undertake sonar operations at different times of day, ranging from morning to darkness, to see if time was a factor in obtaining results.

Surface watching was not emphasized during expedition activities, though still cameras and binoculars were carried by the author and some group members when on or near the lake in the event of a sighting.

A new camera system, built by David Olsen, was brought to the site for testing. It is based around a GBC-1405 low-light, black and white video camera, and powered by a 12-volt DC to 110-volt AC inverter system. However, a faulty Vidicon picture tube was noted during final system bench testing prior to deployment. Tube replacement on site was impractical, so camera trials were canceled for this season.

## RESULTS

The sonar equipment functioned well during the season's work. Some inconvenience was noted during chart paper refill and realignment, and during unit operations at depths right at the thresholds of the automatic phase shifting function (50, 100, 150 feet, etc.), when the continuous bottom tracing would intrude at the top of the chart. Otherwise, this simple unit was well suited for the work.

Numerous bottom features and contours were recorded. Navigational location confirmed that the unit succeeded in recording the deepest section of the Champlain central trough, published on National Oceanic and Atmospheric Administration (NOAA) chart 14783 as 399 feet. Although the unit recorded this basin depression, the depth exceeded its capability for relative scale measurement.

Navigational sightings were successfully taken and used, although refined sighting devices would have yielded quicker and more precise readings.

Sonar chart markings consistent with significant fish activity were frequently obtained, particularly at locations (near submerged shelves and rock outcroppings) and at times (just before and after dusk) predicted in ichthyological and sport fishing literature. Conditions did not allow late night or early dawn sessions this season.

Larger mid-water echoes were recorded on three occasions (July 8, 13 and 18). Although none may be said at this time to represent contacts with large, unknown animals, they merit further investigation, and have been submitted—along with other representative chart recording data from our season—

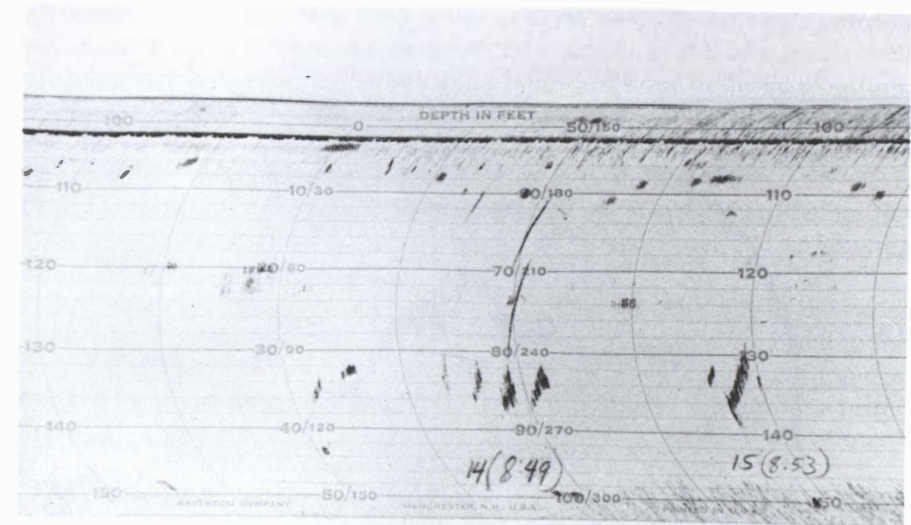


FIG. 1.—A section of sonar chart from the evening of July 18, 1985. The boat was in approximately 105 feet of water, north of Whallon Bay, virtually stationary with little drift. The solid dots and streaks at the top of the chart probably represent fish. The larger marks near the bottom were from reflections at 80 feet plus, as confirmed by observation of the strobe light depth dial. These are of interest, as they are heavy marks ranging near vertically six feet (8:49) and diagonally ten feet (8:53). A weak bottom reflection, possibly from silt, is seen both at the bottom of the chart (range 1, phase 2) and at the top (range 1, oncoming phase 3).

to sonar experts for their comments (Figs. 1 and 2). Hydrophone recordings were successfully made, but yielded no unidentifiable sounds.

No visual sightings of large, unidentified lake animals were made by the expedition. However, information on previously unrecorded sightings was obtained and transmitted to LCPI. An attempt was made to capture a fish (that may have displayed signs of lamprey predation) for its examination by ichthyologists, but the attempt was unsuccessful.

The examination of the McGeoch/Temple sighting at Scotch Bonnet was a first effort of its type but it was hampered by wind and rough water (which made precise shore feature reference sightings difficult), and by the need to divide several tasks (boat handling, boat-to-shore communications, sonar operation, navigational sightings, and note taking) among only two people. However, the experiment yielded significant results.

Although the two women directed the boat separately and without discussion (in fact, Ms. Temple arrived only at the end of the final pass under Ms. McGeoch's direction), and along the course several times, they were



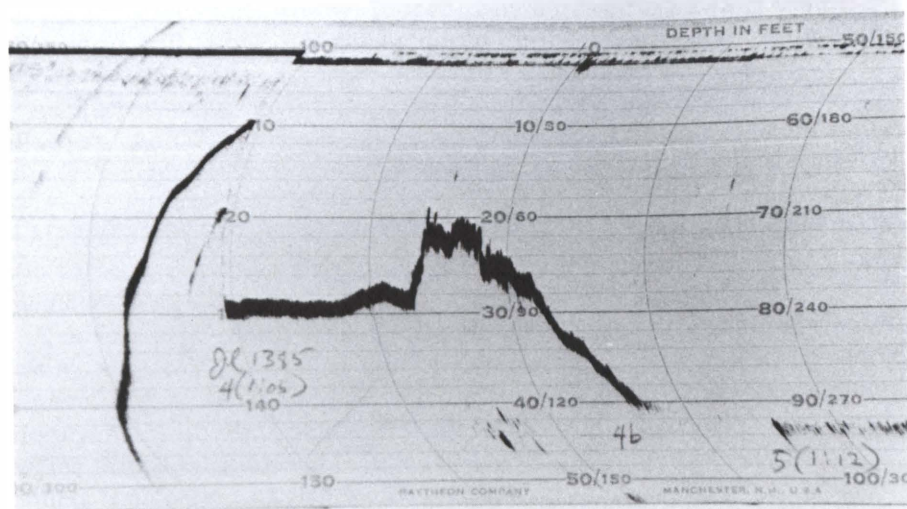


FIG. 2.—A section of sonar chart from July 13, 1985. The boat was under power, headed south past Scotch Bonnet, over a course taken from the Champ sighting of June 29, reported by McGeoch and Temple. The thinner mark on the left shows the rapid decrease in depth from well over 150 feet to 30 feet coming in from the central lake channel. This recording was made at the sonar unit's extreme depth range 3 setting (phase 1). The unit was reset to range 1, showing a plateau feature at 30 feet depth suddenly interrupted by a ten-foot-high rock outcropping. The bottom fell off quickly, so that the signal was temporarily lost, but reestablished at 43 feet.

highly consistent in their positioning of the course and surfacing locations (based on records of the bottom sections being passed over and on what additional surface position sightings could be made from the boat), and in their estimates of the animal's swimming speed (based on motor throttle settings during the experiment; Ms. Temple requested a slightly higher speed). The results of the experiment lend considerable credibility to their report.

It is regrettable that precise measurements of course length against time could not be taken, due to surface conditions and personnel limitations. So, unfortunately, the animal's speed could not be calculated with any accuracy. This problem could be solved in the future by shore use of the new generation of electronic distance measurement (EDM) survey instruments. The swimming speed of the animal reported at Scotch Bonnet, based on the July 24 work, is here subjectively described as a very fast human walking or medium jogging pace.

The sonar records made during all three sessions at Scotch Bonnet (the first two were not under the direction of the eyewitnesses, but based on their

accounts) clearly show that the animal's course past the women's position would have taken it from the deep central channel of the lake basin (well over 200 feet in this area), towards a sharp upward slope in the lake bottom, to a plateau feature (correspondingly, with water depth diminishing rapidly), and, finally, over a reef-like submerged rock outcropping (here the animal surfacing and swimming south, parallel to and approximately 225 yards from shore in comparatively shallow water, 10 to 20 feet deep) before the eyewitnesses lost sight of it (submerging for the last time, and continuing south in depths gradually increasing from 40 feet).

Both eyewitnesses were most definite about the vertical, out-of-the-water undulations of this particular animal. Local sport fishermen reported to the author that the "reef" of Scotch Bonnet (a sharp line of rock outcroppings, marked by navigational warning buoy C"25" in nine feet of water) is a prime fishing area, and chart recording in the area confirmed the presence of many fish.

The tentative hypothesis proposed is that the animal observed was chasing fish over the submerged ledge and rock outcropping features. Its surfacing during this time would be consistent with its need to clear the bottom, if it were a large animal moving in vertical undulations, and/or to momentarily reduce surface drag and thus quickly gain speed for the chase.

The significant matter of the animal's method of detecting the oncoming sharp rise in the bottom (vision? touch? learning? sonar?) is open to speculation. No attempt at explanation is made here.

It should be noted that no feeding behavior was specifically observed, although Ms. Temple reported a side-to-side movement when the animal was underwater, which might be related to predation upon a rapidly dispersing aggregation of fish. Also, the above hypothesis would by no means explain all reported surfacings of such large, unknown lake animals. However, the examination of a claimed route does address important behavioral issues which have remained tertiary, in much of the literature, to proof of existence and classification of type.

This season's work enabled us to gain valuable experience in sonar use and interpretation, small boat handling, and familiarity with lake environs. It is hoped to extend this experience to deployment of the new video camera. As modifications had already been planned for the system, its malfunction this season was not a major set-back.

#### FUTURE PLANS

The author plans further research along the above lines. Emphasis will be given to upgrading the quality of sonar and topographical survey equipment, and the techniques used. Investigations related to Lake Champlain's ecosystem will be undertaken, as it is assumed that the Champ animals, if they exist, are at or near the top of the lake's food chain.

## Book Reviews

*Cryptozoology*, 4, 1985, 80–98  
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*Cat Country: The Quest for the British Big Cat*. By Di Francis. David & Charles, Newton Abbot, Devon, England, 1983. 152 pp. £5.95 (c.). (North Pomfret, Vermont, 1983, \$12.50.)

In the introduction to this book, which argues for the existence of a large puma-like cat exclusive to mainland Britain that has evolved separately from other known species, the author states her reasons for writing *Cat Country* as being “to try and prove it.” For me, professed cryptozoologist and one-time pursuer of Africa’s legendary King Cheetah, she has singularly failed to do so.

Perhaps a clue to this failure may be found elsewhere in the same introduction. In its very first sentence, Francis makes clear she is a journalist. With respect to that worthy profession, I cannot help but feel that *Cat Country* is little more than a carelessly slung-together sacrifice to commercial journalism, lacking sound scientific argument. With its evidence clouded in “journalese,” it fobs the issue, and falls a long way short of being convincing. This is more than evident in the lack of scientific support in the author’s judgments, arguments either frustratingly not put forward or not thought through, getting straight scientific facts wrong, and logical comparisons rarely, if ever, drawn, thereby negating the opportunity for any search for ideas. Such material hardly justifies the book’s inherent antagonism toward scientists.

What the book does provide, and herein lies its value, is a hitherto undocumented catalog of “eyewitness accounts” to date of sightings of “big cats” of varying sizes and colors over a period of a number of years. Unfortunately, with sightings generally ranging through lion-like, puma-like, lynx-like, even tiger-like and cheetah-like way down to thylacine-like, there is a blatant absence of a complimentary analysis to the sort of degree that the discrepancies and contradictions between the sightings demand. It is not, to take Francis’ line, that a bevy of respectable, sensible, educated people could all be either mad, drunk, or liars. It is simply a proven fact that many sensible, respectable, educated people can, when not prepared for a sight they are not expecting, be notoriously incorrect in their observations—notably in the dead of night, and especially of the kind of details Francis keeps

drawing our attention to, i.e., the color of the tip of a tail, whether the ears are rounded or pointed. To ignore this is plainly naive. It is enough for a great many of the eyewitnesses that what they saw was a “bigish” cat-like animal that varied from medium-dog size—terrier size, say—to Great Dane size. All the more reason for a deeper, more objective analysis of such differences.

I find the photographs and casts taken of pawprints equally unconvincing proof for the existence of a bigish cat exclusive to Britain—other than that of the native wildcat, that is, which at one time inhabited all England and Scotland. By the 1850’s, it was virtually exterminated in all but Scotland, the Welsh Mountains and its environs, the Cumbrian Mountains, and the Lake District—the very areas of the “big cat” sightings of the present—coming to the point of almost total extinction by the turn of the century. Since the world wars, however, with the absence of gamekeepers during the 1914–18 period, followed by a more enlightened attitude since then, the wildcat has enjoyed a comeback, notably in Scotland. Hence it is not entirely illogical to suppose that the same might be happening elsewhere in the remaining remote areas of mainland Britain where it once flourished. Neither is the appearance of what are thought to be feral domestic-wildcat crosses unknown, possibly accounting for long-legged, pure black offspring measuring upwards of 4 feet in length.

It is this that brings me to one of the book’s more glaring omissions: the author’s failure, or unwillingness, to discuss to any significant degree the possible connection between many of the sightings and the wildcat whose increasing comeback—exciting from the broader zoological viewpoint rather than the cryptozoological—cannot on the evidence cited be in any way discounted from the argument, certainly with the enormity of difference in description the “big cat” sightings indicate. But the author has done just that.

The descriptions in many of her listed sightings also fit wildcats. Even the two most revealing photographs in her book (pages 110 and 112) depict what looks to be, without question, wildcat, from the ears and white throat right through to the tip of the tail. Even more noteworthy about the same photographs is that, while purporting to be two quite separate kittens, close examination of the pattern of each indicates one and the same animal. All patterned animals have markings distinct to themselves, like fingerprints, allowing for just this type of check. I also question it being a kitten. The whole mien of the cat speaks of a confident, experienced animal. The mature lie of the shoulders and the cat’s total concentration on the bait—neither are that of a skittish young kitten still attached to mother: a mother cat which, in any event, would hardly allow her young to go to bait without first investigating or disturbing it herself. Nothing in the text or photos indicates that she did—though we are led to believe she was present the night the



photographs were taken. Thus, both in size and pattern, we are left with a cat looking unequivocally like a wildcat, if not suspiciously like one.

Another important discussion remains, and that relates to the evidence of the casts of pawprints reproduced in the book. The author argues for the existence of a large, puma-like cat, perhaps surviving since Pleistocene times; a true living fossil that has survived the traumatic arrival of man by taking to the trees. An arboreal animal. A good argument. Well, it would be if it were not for the presence of claws in all three cast-prints, claws that are clearly placed, as in dog pawprints, and they are certainly not those of an arboreal cat that is reputedly the size of a puma. The lions of Tsavo, though they may take to low scrub trees, can never be classified as "arboreal" by virtue of their size, which restricts their tree-climbing agility.

On the other hand, the cheetah, the only cat to regularly show indentations of claws in its normal spoor, is restricted from being even remotely tree-dwelling by virtue of those very claws, which, along with other characteristics, are specifically related to its unique adaptations as a high-speed sprinter.

Thus, for any puma-sized Pleistocene cat to have survived to present times, unknown and virtually unreported in this overcrowded, sophisticated, hunting/shooting/poaching-mad countryman's island, where even the smaller predatory wildcat has been virtually exterminated, it would really have to hang in there in what remnants of British forest remain, with the high-flying agility of the acrobatic gibbon, which, alas, the supposed size of the animal cited by the claws in the cast-prints as much as logic, belie. For example, not one skin in a period of some 1,000 years or more? No reports when the population of the same island reduced a smaller cat to the point of virtually killing it out? How much more so a bigger cat, supposedly the size of a puma, which doubtless wouldn't restrict its kills to the type favored by the smaller wildcat, its sheer size dictating a need every 2 to 3 days to bring down far bigger game than the rabbits, pheasants, or lambs sought by the wildcat. The potential depredation on large livestock, not least the stag and deer population, would have driven farmers and gamekeepers down the centuries to desperation, ever-vigilant as the British countryman has always been to the presence of marauding predators. Witness, again, the near-extermination of the wildcat on the grounds that it was "vermin," and thus a threat to stock.

Are we seriously to believe, then, that those same vigilant British countrymen wouldn't have inflicted a similar fate on a bigger, puma-like predator, with all the resultant evidence for the existence of such a cat that would bring? Even in naturalist Victorian times, with their penchant for shooting and stuffing as many specimens as the museums and private collections would allow, it is difficult to accept that the existence of a big predatory cat

the size of the puma, and unlike any other known species, would actually go unnoticed to the point of being scientifically unknown.

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*The Loch Ness Mystery Solved.* By Ronald Binns. Open Books, Shepton Mallet, Somerset, 1983. 228 pp. £7.95 (c.). (Prometheus Books, Buffalo, New York, 1984, \$18.95.)

The spring of 1983 saw the fiftieth anniversary of the Loch Ness Monster controversy, and, perhaps inevitably, the publication of another book on the subject. This time, however, we see the question examined by a former believer who now reaches an opposite and uncompromising conclusion.

Binns' preface provides a manifesto of the contents. The basic problem is that the mystery *has* endured for fifty years, despite all research efforts, within a finite body of water. For the most part, he does not doubt the sincerity of those reporting inexplicable experiences, but draws attention to the fact that the more research takes place, the less evidence results. He and R. J. Bell (who assisted him) recall how, as students twenty years ago, they considered themselves part of a band of dedicated amateurs at odds with an indifferent scientific establishment. After ceasing in 1972 to be involved in active research, Binns believes he has kept in touch with developments. The reader is invited to join in an inquiry into the history and natural history of Loch Ness, the sources of the legend, photographic evidence, and the findings of underwater equipment.

The quest begins with the history of the loch before and after the press sensation of 1933. Here Binns is at his best. He has become aware, as have other researchers, that an examination of monster literature reveals many inconsistencies and errors. He underlines one of the most suspicious elements in the monster story, namely the apparent suddenness of the phenomenon. The pro-monster lobby would normally explain this by pointing out that a new road had recently been built along the shore of the previously remote and inaccessible loch. They would also indicate a succession of sightings recorded in literature dating back to *The Life of Saint Columba*, written by Adamnan in 565 A.D.

A carefully researched exposition by Binns shows that the references quoted by other authors are dubious, distorted, or completely non-existent. He

makes rather a meal of the information that St. Columba's encounter took place, not in the body of the lake itself, but in its river outlet, and his point that such accounts were not designed to be factually accurate seems a little pedantic. However, it is clear that the authors attacked by Binns, who were his contemporaries during his early field research, did not check their references.

The author's carefully compiled history demonstrates that the loch, far from inaccessible, has been central to the Great Glen routeway for a very long time. The new road of 1933 was actually a series of improvements to a much older one. However, Binns does not really demolish the argument that motor traffic increased, and visibility became greatly improved, by the cutting down of trees.

What is demonstrated in the book is an absence of reliable references to the Loch Ness Monster before 1933. Binns ignores retrospective accounts, deliberately sidesteps the Highland Water Horse tradition, and excludes discussion of "monster" sighting records from other lochs; but, then, the book is about Loch Ness.

An important point raised by Binns is the influence exerted by just one man. Alex Campbell, Loch Ness water bailiff and correspondent for the *Inverness Courier*, not only initiated the first newspaper reports, but contributed the all time classic account of a thirty-foot plesiosaur disporting itself as the mist cleared from Fort Augustus on a June morning in 1934. Students of the literature should be aware that this sighting is first recorded by Gould (*The Loch Ness Monster and Others*, Geoffrey Bles, 1934) as occurring in 1933. Subsequently, Campbell retracted the sighting after having seen the effect of mirage on a group of cormorants. The sighting was then revived, redated, and from time to time redrafted by others. Though hardly the first to raise these issues, Binns provides a well laid-out and thoroughly researched argument. In the library, he is in his element.

The way in which further "evidence" appeared is then examined. Firstly, a very dubious land sighting fuelled the sensation, and then a succession of "classic" photographs were produced, right up to the present day. They differ widely, and in many cases can be shown to be hoaxes. Binns then demonstrates that organized watching throughout the 1960's produced no results resembling the classics. Other evidence is then tackled, such as the Dinsdale film of 1960, and the results of underwater research. He dismisses all of it.

This still leaves the surface eyewitness sightings, of course, and a discussion then covers the general unreliability of such evidence, the selectivity of authors in quoting accounts conforming to the stereotype, and the ways in which witnesses can be deceived. Drawing on his experiences as a former monster watcher, Binns lists causes of error, such as mirages, windrows, and boat wakes. He also suggests animals such as swimming deer, otters, birds, and fish. Strangely, he omits seals, one of the more likely candidates. He

concludes by showing the public need for monsters, and the private thirst for adventure in a shrinking world. All researchers are characterized as having little interest in zoological matters, and belonging to a "fringe religion" awaiting the humiliation of the establishment.

This second half of the book dealing with evidence, though well written, betrays a lack of technical knowledge, a shortage of practical research, and a complete ignorance of current attitudes. The attack upon former comrades-in-arms has a bitterness which spills over into the rest of the work. Clearly, if there is no monster, then all evidence must be accounted for somehow, and human frailties must answer for a great deal, but Binns unfortunately seems to prefer personal innuendo than to an assessment of material. To help in this, he makes extensive use of newspaper quotations. In dealing with the classic photographs, he draws heavily upon the conclusions of others, but gives them scant credit elsewhere for their mounting skepticism. A handful of photographs of rocks, birds, otters, and a fake monster are certainly worth inclusion, but fail to conceal a lack of effort in reaching the truth of individual pieces of evidence.

A central chapter on the Dinsdale film of 1960 shows that the conclusion in the report by the Joint Air Reconnaissance Intelligence Centre (JARIC), Royal Air Force, that the object is "probably animate," to be conditional on the observer being able to see that it was not a motor boat. However, Binns then complains that the analysts had not "bothered" to make on-site estimation of the height of the camera position, which they regard as crucial for the measurements. He could have worked out for himself that Dinsdale had forgotten to allow for the loch being 52 ft above sea level when he read his height from the map. The 52 ft error in the base data did not, in fact, prevent JARIC making some common sense estimates on a known fishing boat filmed later for comparison. This reveals the wide margins of error in the calculation of down range speeds, and is the key to the real weakness of the report. Binns attacks JARIC for its failure to identify the World War II German rocket launching sites at Peenemunde, and also speculates at length about Dinsdale's presumed physical and psychological condition at the time of filming. He cannot even resist a dig at zoologist Maurice Burton, a believer turned skeptic, stating that "Burton was undoubtedly wrong in identifying the mystery object in Dinsdale's film as a local fishing boat." JARIC makes a distinction between motor boats and fishing boats. Neither Binns nor JARIC examined the film on video. A little contrast adjustment as the "wake" passes across the field of view is all that is required to determine whether Burton was right.

When dealing with the Academy of Applied Science underwater results of 1972, which included a sonar trace and the well-known flipper pictures, Binns states that the camera was triggered by the sonar, when, in fact, there was no connection between the two. Having discarded his chance of making



a valid criticism of the results, he is reduced to suggesting that the area was "frequented by frogmen," and that this is a good time to discuss the Piltown Man hoax. Had he followed his earlier practice of examining the literature for inconsistencies, he would have found, for example, the assertion that the sonar transducer was fixed to the loch bed to be contradicted by the expedition members' statement that the target was lost when the boat swung to a breeze.

When discussing sonar, the best Binns can suggest is that it doesn't work. In cataloging the various reports, it is clear that he is simply out of his depth. Not only does he make numerous errors of detail, but misses most relevant clues to what would have been valid criticisms.

This book is not really concerned with the largest creatures to be found in Loch Ness, since there is no information given on the environment. There are no surprises for current researchers, who, in fairness to the eyewitnesses, are tackling the remaining questions. The shortcomings of the book are due, perhaps, to the deadline imposed by Nessie's fiftieth anniversary, the race to be the first, and a jealous determination to seem alone in its conclusions.

For the public, however, the laying to rest of the monster stereotype of the 1960's is long overdue. If it is, at the same time, a little premature, it is unlikely that too many people will notice.

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*Mysteries: Encounters With the Unexplained.* By John Blashford-Snell. Bodley Head, London, 1983. 251 pp. £8.95 (c.). (Merrimack, Salem, New Hampshire, \$16.95.)

This volume is a fascinating compendium of the author's explorations and investigations in different parts of the world. It is written in a lively, personal style, with many anecdotes and a good touch of dry British humor. Col. John Blashford-Snell—or "Blashers," as he is known to his friends—leads a charmed life. He is an officer in the British Army's Royal Engineers, Chairman of the Scientific Exploration Society, and Chairman of the British Chapter of the Explorer's Club. Armed with a Smith and Wesson .38 and a bottle of J&B scotch, he somehow becomes involved in one adventure after another, usually in some far-off land, while studying its geography, zoology, or history.

Believing that there are yet many natural wonders to be discovered, or "mysteries" to be uncovered, Blashers represents a sort of living embodiment of the Victorian spirit of exploration, pith helmet and all; but he is not hesitant to use modern, 20th Century equipment—such as short-wave radios, helicopters, and Avon inflatables—when he needs them. In the 1970's he directed Operation Drake, a scientific voyage which followed the route of Sir Francis Drake in the Golden Hind 400 years ago, and which involved the participation of about 250 young volunteers.

The present book, the author's eighth, recounts archaeological work in Panama and underwater archaeology off of Cyprus and Bimini; explorations of volcanoes in Kenya and St. Vincent; studying unique "cave" elephants in Kenya; exploring Papua New Guinea, including the finding of a long-lost World War II U.S. fighter aircraft; exploring a 100-mile long gorge in Ethiopia, through which the Blue Nile runs (and during which they were surrounded and attacked by bandits—with Blashers' .38 blazing, they narrowly escaped with their lives); the finding and identifying of a mysterious, crashed B-24 in the Libyan desert; exploring wartime German tunnels on his native island of Jersey; and several very British ghostly encounters.

While none of these topics deal with unknown animals, persons concerned with cryptozoology will almost certainly find them highly interesting. Two chapters are specifically cryptozoological. The first deals with Loch Ness and Loch Morar. Blashers arrived at Morar in 1976, hoping "to find an area on which to train my regiment the following year," and got to know and to work with Tim Dinsdale and Adrian Shine. This association culminated in 1982 with the deployment over Loch Ness of the Goodyear blimp *Europa*, normally based in Italy.

The *Europa* flew up and down the loch in early June of that year, working in conjunction with Shine's research boat, which was using sonar. Flying low, those aboard *Europa* hoped they would be able to see and film Nessie from above if and when Shine's sonar detected and pinpointed its location. On one occasion, while Blashers was operating a control radio in the Urquhart Castle ruin, Shine's crew did track a large, subsurface target. However, this was not visible to Dinsdale and the others, hovering 250 feet above, because the target was about 50 to 60 feet below the surface (visibility in Loch Ness is reduced to only a few feet because of suspended peat particles). Nevertheless, a good time was had by all, and the mission—the 1982 Great Glen Airship Expedition—succeeded in a number of other scientific experiments conducted by the Highland River Purification Board, the Forestry Commission, and Aberdeen University.

Blashford-Snell is convinced that Nessie represents a large, unknown animal, although he takes no particular stand on its zoological affinities. He asks if it could be a huge amphibian, a giant mammal, or a large sturgeon, adding that Sir Peter Scott's plesiosaur hypothesis "cannot be dismissed."

The second cryptozoological chapter concerns his search for a legendary dragon in Papua New Guinea named *artrellia*. It was reported as a huge, tree-climbing "crocodile" that can eat people—and has. Exploring deep in the southwest part of the country, he deployed night patrols in the appropriate areas indicated by the natives, with no results. Finally, as they were leaving, an *artrellia* was caught, vindicating the native tales, even if these were exaggerated. The animal was a large, 6-foot monitor lizard, *Varanus salvator*, a close relative of the Komodo dragon. Expedition members believe they observed 12-foot specimens, and Blashford-Snell is certain there are 15- or 20-foot giants, still unknown to science—and these certainly *could* be man-eaters. It is not clear whether these would be *Varanus salvator*, or another, still undescribed species, but the author asks soberly: "How big do they grow? Perhaps Operation Raleigh will find out in 1986."

And that is exactly what he now hopes will happen, as he is currently leading this new expedition around the world for several years.

Operation Raleigh, with the patronage of H.R.H. The Prince of Wales (Prince Charles), includes the participation of hundreds of volunteers from many different countries. The flagship of the expedition, SES *Sir Walter Raleigh*, has already been to Central America and the Caribbean, and is currently operating in South America. It will be going next to Australia and New Guinea.

Maybe Blashers will finally get his chance to bag the giant *artrellia*—and then stop for a spot of tea in the jungle to celebrate!

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*Monsters: A Guide to Information on Unaccounted-for Creatures, Including Bigfoot, Many Water Monsters, and Other Irregular Animals.* By George M. Eberhart (Comp.). Garland Publishing, New York, 1983. 344 pp. \$25.00 (c.).

Wouldn't the media have a field day if a real, verifiable "sea serpent" emerged in New York City's harbor, or Canadian lumbermen dragged a live Sasquatch out of the wilderness in chains? Such events would be emotionally equivalent to a UFO landing on the Mall in front of the Smithsonian! In actuality, the media have been having fun with "monsters" for many years. This massive bibliography by George Eberhart makes this very clear.

Eberhart's book bears the main title *Monsters*, but it really deals primarily with: 1) animals not yet accepted as real by the scientific community, such

as the Loch Ness Monster; 2) animals that are much larger than those listed in zoological records; and 3) familiar animals that are found far outside their normal ranges, such as alligators in New York sewers. Excluded from this book are such things as calves with two heads, which are also called "monsters" by some.

The adjective "Fortean" must be applied to Eberhart's book. It is strong on Bigfoot, large unknown aquatic animals, "phantom" panthers, giant snakes, surviving dinosaurs, and the like—all exciting fauna to be sure, and also sure to raise the blood pressure of many conventional zoologists. In other words, Eberhart pretty much confines his bibliography to animals that would be spectacular and/or embarrassing to science *if* they were found to really exist. Such is the essence of Forteanism. One notes that giant bacteria, unrecognized viruses, and geographically displaced insects are not included, even though these types of life forms dominate our planet in terms of their numbers. They are simply not exciting enough. To a large extent, modern cryptozoology parallels Forteanism in its scope.

But all worthwhile projects have to begin somewhere. Eberhart's *Monsters* is a good embryonic data base of biological anomalies, just as cryptozoology is a valuable preliminary infrastructure for socially organizing the pursuit of biological anomalies.

Focussing again on *Monsters*, this encyclopedic bibliography is a remarkably good collection of references to "Fortean zoology," replete with perceptive observations and overviews at the beginning of each chapter. The chapters are species-oriented: African dinosaurs, freshwater monsters, giant snakes, humanoids in Asia, merfolk, "phantom" kangaroos, etc. There are 4,450+ numbered references all told, listed alphabetically by author. After the author's name, the reader will find the title of the article (or book), the journal/magazine (or publisher), and the volume number and/or date. One wonders at the immense labor Eberhart expended in locating all these marvelous articles and tidbits. The book is a masterpiece of literary nuggetry, but, of course, completeness is not expected in such a large endeavor. The collection is almost exclusively from English-language publications. Author and source indexes complete the book, and add to its usefulness.

The best sort of recommendation this reviewer can give is to state that he has, in fact, read *every one* of the 4,450+ entries in an attempt to determine whether it might be worthwhile trying to acquire some of them. "Trying" is an appropriate word here because many of the books and periodicals given are most elusive, even when major libraries are near at hand.

The general nature of the sources in *Monsters*, as in much Fortean research, poses a problem—a credibility problem. While even top scientific journals, such as *Nature*, may on rare occasions present inaccurate or even phony data, one is much more likely to find such "noise" in data bases which are based mainly on newspapers, travellers' tales, and collections of folklore.



Eberhart's source index reveals that most of the references are not from refereed, mainstream scientific journals. Now, this is not surprising, because scientists generally avoid anything verging on Fortean. However, despite this noisy data base, many strong signals come through if one applies proper filters. The strength and variety of these signals make *Monsters* a valuable volume for any cryptozoologist.

As a generalist and anomalist, I like Eberhart's *Monsters*. I like the thought that dinosaur-like animals may still be wallowing in African swamps, and that humanoid eyes may be watching hikers in the Pacific Northwest. Ideally, Eberhart's approach should be extended to all biological anomalies—to all species, to anomalies of behavior and sensory capability, to evolutionary quirks, to the fossil record, and to that murky borderland between life and nonlife.

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*Living Wonders: Mysteries and Curiosities of the Animal World.* By John Michell and Robert J. M. Rickard. Thames and Hudson, New York, 1983 (London, 1982). 176 pp. \$9.95 (p.).

In *Living Wonders*, British writers Michell and Rickard stroll through the literature of the bizarre, and draw freely from the works of scientists and cranks (usually showing a preference for the latter) to "explain" the phenomenon of cryptozoology and of other (purported) natural anomalies. Michell and Rickard note, for example, the cryptozoological speculations of Hans Horbiger, a catastrophist who believed that periodic close approaches of astronomical bodies caused disturbances in the earth's gravitation, producing giant animals, insects, and plants. "It followed from Horbiger's view of past processes," the authors observe, "that the same would one day be repeated and that gigantic animals would again roam the earth."

They further speculate as follows:

The . . . monsters and mystery creatures sighted today . . . all represent apparent revivals of archaic, long-extinct types which science has only revealed to us within the last hundred years or so. For ages after their physical disappearance from earth, the shades of atavistic creatures have continued to haunt their former regions and infiltrate the legends of the local people. Now, it seems, they are becoming more resistant. It could be that there are revivals in the offing . . . Encouraged by popular acceptance and expectation, a thought-form, say of a giant reptile in Loch Ness or a King Kong-like ape in the North American mountains, may become established. From there it is only a short step for it to become real—as far as anything is real in this curious world—and to present its anatomy to science.

The authors borrow from Darwin, Sir Richard Owen, Cuvier, Jung, and Maurice Burton, whose ideas they find, on occasion, to be as interesting and as useful as those gleaned from the likes of journalist John Keel, chronicler of "Mothman," who holds that monsters, ghosts, and assorted other entities enter the world through "windows" leading to and from other dimensions.

This is quite a wonderful book, written by two literate, iconoclastic, funny men who, unlike many of those they mention with mock approval, are only masquerading as crackpots. Their mentor, of course, is humorist and anomaly collector Charles Fort. In this, the best Fortean book since Fort's last (*Wild Talents*, 1932), Michell and Rickard argue, with charming comic detachment, that a crank's illusions can be as elegant (and, in their own crazy way, as compellingly logical) as a scientist's truths; moreover, being open to aspects of experience (physical and psychological) with which science has yet to find a way to deal, the crank has a larger vision of the world. He may misunderstand what he sees, but at least he *sees*. And what he sees, in this case, are nature's anomalies, which hint at new definitions and anarchic possibilities that would horrify more sober thinkers.

Michell and Rickard leave it for others to wonder about such pedestrian matters as the validity of anecdotal testimony, the reliability of memory, the likelihood of leg-pulling—in large part, no doubt, because this would get in the way of the fun. Another reason, however, is that too often these explanations, while reasonable enough in most cases, are employed as an easy way of dodging difficult questions.

Of course, many of the phenomena here are surely not manifestations of nature as such, but of *human* nature, which spawns legends, memorates, hallucinations, dreams, and lies, and lets them pretend to be facts. But much remains, including the stuff of cryptozoology, to which Michell and Rickard devote the 60 pages of Part One.

They give us an excellent concise history of cryptozoology (and well-illustrated, too), showing their wide knowledge and their grasp of significant issues. They are not, it need hardly be said, writing as scientists; in this section, as in all the others, their point of view is strictly Fortean. The first chapter reviews, in engaging fashion, the evolution of cryptozoological literature. Readers who come to this from a strictly scientific perspective, will doubtless be unsettled to see the works of paranormalists treated as respectfully as the books of Heuvelmans, Mackal, and Ley, and to find a depiction of the "Owlman of Cornwall" situated not far from illustrations of giant anacondas and giant squids.

But in the larger world visible to the crank, the iconoclast, and the perversely amused, this world's zoological Sasquatch (Oregon, Washington, British Columbia) and the twilight zone's apparitional Sasquatch (points east and south) coexist, both in bodies of testimony and in corpora of (generally ambiguous) physical evidence. If the Northwestern Sasquatch makes a cer-

tain tenuous scientific sense, and the Midwestern/Eastern Sasquatch none whatever—well, so it goes. In any case, neither Sasquatch, real or imagined, is likely to go away soon. Both cryptozoology's critics and its less-inhibited advocates are going to be asking the inevitable questions before long.

If only to anticipate these questions, serious students of cryptozoology who don't already know should learn what their subject looks like to those unbound by scientific constraints, be they the authors of *Living Wonders* or "witnesses" to cryptozoological specimens in unacceptable places.

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*Mysterious America*. By Loren Coleman. Faber and Faber, Winchester, Massachusetts, 1983. 301 pp. \$9.95 (p.).

Shortly after seeing his father's ghost, Hamlet remarks to a friend: "There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy."

There are a lot of strange "things" happening in these United States (and other American countries), and a goodly number of them are described in Loren Coleman's *Mysterious America*. While not all are cryptozoological in nature, there are enough "mystery animals" here in the U.S. to keep interested readers busy should they want to spend their vacations hunting for the beasts.

However, I must note right up front that, as most cryptozoologists are not Fortean, some readers may be put off by Coleman's book. It contains not only accounts of such well known crypto-critters as Bigfoot and Champ, but a host of other reports of high strangeness: out-of-place animals, "phantom" cats, mystery kangaroos, mad gassers, even The Jersey Devil himself. In short, it's a potpourri, with something for almost any lover of the strange and the unusual.

There are five chapters (out of 22) which deal largely with the crypto-creatures with which most of us feel comfortable: Giant Snakes, Lake Monsters, Champ, Bigfoot, UFO's and Momo, and The North American Ape. Where possible, Coleman has visited and talked to eyewitnesses. When this has not been possible, he has relied on the work of other investigators, as well as news accounts of the incidents involved. The end result is, basically, a collection of anecdotal accounts which, while not supplying the sort of proof that would be acceptable either in court or to a "jury" examining papers for a scientific journal, is, nonetheless, quite fascinating. We either have to

assume that a great many folks are utterly mad, totally incapable of knowing what they see, or are liars. While it may be more comfortable for us to believe them liars or incompetents, we must continue to remember that the zoos and museums of the world are full of specimens first described by "uneducated" natives, but disbelieved by scientists until some hunter or circus collector dragged an actual specimen out of the bush.

The difficulty with belief in mystery animals in the United States, is, of course, that it is not, in the popular mind, a wilderness land where a breeding population of unknown creatures can live. Yet, lake and land monsters continue to be reported.

Take Champ, for example. Lake Champlain is over 100 miles long, 13 miles wide, and up to 400 feet deep. It protrudes into Canada, is connected to the sea, and has, since 1609 when Samuel de Champlain made a note of his sighting, had a written history of large, unknown, aquatic creatures in it. Sighting reports have come from both individuals and groups, both on shore and afloat. As the lake freezes over entirely during some winters, the connection to the sea may be important. Some, like Roy Mackal, believe Champ is a primitive whale, a relic archeoceti which "commutes" seasonally between sea and lake, thereby eluding the winter ice-pack breathing problem. Others, including Richard Greenwell, lean towards surviving groups of warm blooded plesiosaurs. Presumably, they would also commute in season.

Coleman discusses the history of Champ sightings, reprints the Mansi photograph, and concludes that "the Lake Champlain Monster appears to be an actual creature frolicking in the autumn mist."

On land, the shadowy figure of Bigfoot, says Coleman, stalks not only the Pacific Northwest, but such eastern states as Pennsylvania and Florida, as well as mid-America: Arkansas, Missouri, and Illinois, for example. Again, the reports are anecdotal, but the few photos (notably the Patterson-Gimlin film from northern California) are augmented by casts and photos of tracks in soil, dust, and snow.

South of the United States is the domain of the giant snakes. (I'll ignore here Coleman's accounts of pythons and boa constrictors found in homes and yards in the U.S., though there is no doubt that the usual authoritative solution—"escaped from a zoo or circus van"—doesn't always hold up when examined closely.) Central and South American reports—again regrettably anecdotal—tell of anacondas up to 70 feet long, as well as reports of unidentified snakes observed athwart trails, and of such length that neither head nor tail could be seen. Clearly, reports of such gargantuan reptiles will continue to defy belief until an actual specimen is obtained.

When will that occur? Who knows. Few expeditions exist today searching for such creatures. While a few cryptozoologists have penetrated parts of Africa searching for Mokele-Mbembe, the cost of such ventures is far higher than most groups of enthusiasts can fund, and benefactors are hard come



by these days. Neither circuses nor zoos, two main sources for expedition funds a century ago, have the cash to spare, while nationalistic fervor and general back-country unrest in the developing parts of the world, where giant snakes and swamp monsters are said to live, make hunting difficult even if money is found.

Frankly, I think this makes Coleman's book all the more important. Modest funding within the contiguous 48 U.S. states can make Bigfoot and Champ expeditions possible—and successes in either or both of those searches could loosen purse strings for larger efforts.

Should one wish to plan one's own expedition, Coleman's Appendices, which include one on Lake and River Monsters, plus a Regional Bibliography of books about America's mystery creatures, will be most useful. I do wish, though, that an index had also been included.

So read *Mysterious America*, extract from it the crypto-critter data you feel comfortable about, and ignore the rest—you'll still have ample mysteries to chew on, and, hopefully, to investigate.

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*Wildmen. Yeti, Sasquatch and the Neanderthal Enigma.* By Myra Shackley. Thames and Hudson, London, 1983. 192 pp. £7.50 (c.). (*Still Living?* Thames and Hudson, New York, 1983, \$16.95.)

This book is a survey of reports of unknown, primate-like animals from North America, Central Asia, the Himalayas, China, and Siberia. One group of these animals is thought by the author to be descended from the fossil ape *Gigantopithecus*; another group is thought to be composed of relict hominids—that is, hominids which are not anatomically modern and are known only from fossil evidence, which have managed to survive in isolated areas to the present. Specifically, Shackley, an archaeologist, argues that the taxon of fossil hominids known as the Neanderthals is responsible for many sightings of unknown animals in Central Asia.

Neither *Gigantopithecus* nor Neanderthal survival as an explanation for reports of unknown animals is an idea original to Shackley. From the prologue, the book appears at first to be a continuation of the works of Heuvelmans, Porshnev, and Sanderson, with a great reliance on Chinese and Russian sources mostly unavailable in the West. However, it soon becomes clear that Shackley does not employ a true scholarly approach to the zoological data, and that there are major flaws in her paleoanthropological and archaeological interpretations.

The book begins with a chapter detailing Greco-Roman myths of fauns and satyrs, medieval tales of forest wildmen, and stories of feral children from the 18th Century to the present. This survey is presented because of the possibility that, if relict hominids do exist, evidence for their survival may be preserved in certain cultures in the guise of these myths and tales. It soon becomes apparent that this survey deals with myths alone, or true, isolated instances of feral children, and not relict hominids, as Porshnev had hypothesized.

The major portion of the book then follows, with sightings of unknown, primate-like animals divided into geographic areas. Shackley's reporting is second- or third-hand. Three groups of unknown species are apparent: the Chuchunaa from northeastern Siberia; the Sasquatch or Yeti from North America, the Himalayas, the Pamirs, Siberia, and China; and the Almas from the Caucasus Mountains eastward to Mongolia. The old Chuchunaa sightings seem to be of undoubted humans, tall hunter-gatherers wearing hide clothes, using bows and fire, and now, presumably, extinct or assimilated.

The Sasquatch or Yeti reports occur principally in three chapters, for North America, the Himalayas, and China, but are also scattered in other chapters as well. Shackley contends that these creatures are descendants of the fossil ape *Gigantopithecus*, a genus which first appeared 7–9 million years ago in Pakistan and India, but which survived into the Pleistocene in China. The genus is known only from abundant tooth and jaw remains, and possibly a fragmentary lower humerus from Pakistan. Shackley is not the first person to connect *Gigantopithecus* with the Yeti or Sasquatch, but I emphasize that no proof whatsoever exists for this connection. There is no fossil or skeletal evidence detailing such an evolutionary progression—in fact, no one knows for certain about the postcranial anatomy of *Gigantopithecus*, or even what locomotion was like in members of this genus. The undoubted existence of *Gigantopithecus* is used as an argument for the existence of the Sasquatch or Yeti, once an ancestor-descendant relationship is assumed. The end of chapter two is riddled with problems: if the nearest living relative of the Sasquatch is the mountain gorilla (p. 45), this negates Shackley's claim of its descent from *Gigantopithecus*; a single molar tooth of *Purgatorius* (p. 48) is not the entire fossil record of non-human primates in North America—in fact, primates abound in the early Tertiary of North America, and probably originated there; if the ancestors of the Sasquatch reached North America from the Old World prior to the beginning of the Ice Age (p. 49), then how could the Bering land bridge have been crossed, when global sea-level falls are dependent on the growth of continental ice sheets?

Shackley's review of the evidence for Sasquatch or the Yeti would not persuade open-minded individuals of the existence of an unknown type of animal. There is no sober examination of the zoological evidence that known

mammals may be responsible for Yeti sightings. For example, rather clear evidence that the Whillans Yeti sighting of 1970 (p. 59) was based on the observation of a langur monkey leaping along on all fours in the moonlight is not discussed. As a result, Shackley concludes (p. 62) that almost three-fourths of the Himalayan reports deal with a creature which is quadrupedal, especially when moving quickly or in deep snow. She never considers that known mammal species may perhaps be responsible for most of these reports, and is forced to conclude that, for some reason, the Chinese Yeti and North American Sasquatch are more bipedal than the Himalayan forms. Evidence that some of the Chinese Yeti sightings may be based on the observation of golden langurs (*Rhinopithecus roxellanae*), as proposed by Frank Poirier, is also not reported.

This degree of credulousness with regard to the Yeti and Sasquatch does not engender confidence when the author finally deals with the Almas, presenting her arguments that the Almas are really Neanderthals surviving in Central Asia. This is the heart of the book, and includes Shackley's own archaeological data on Mousterian tools found in Mongolia. I count six points which the author adduces to demonstrate that Almas are Neanderthals, and are surviving to the present.

1) The sympatry or coexistence of early hominid species makes the survival of Neanderthals more likely (p. 11). Actually, whether species in a family of animals coexist at a particular time has nothing to do with the likelihood of coexistence and survival at a later time; later coexistence is determined by the current evolutionary situation. 2) The overlap of areas where Almas have been reported and known Neanderthal sites is significant (pp. 8–9). Two Neanderthal sites occur in this overlap, and only one (Teshik-Tash) actually contains Neanderthal skeletal material, which would make occupation by the Neanderthals unequivocal. How can this be a significant association? 3) Stone tools of the Mousterian industry absolutely indicate the presence of Neanderthals. On the contrary, there are a number of instances in which hominids that are not Neanderthals have been found in association with Mousterian implements, such as the sites of Jebel Qafzeh and Skhul, and the Neanderthal burial at St. Césaire was associated with the Chatelperronian industry.

4) The surface collections of Mousterian implements made by Shackley, and dated by her to perhaps even more recent than 20,000 years ago (p. 162), indicate the very late presence of Neanderthals in the Altai foothills of Mongolia, especially because many of the tools are "fresh and surprisingly unworn." Despite this section representing Shackley's own work, there are no photographs, line-drawings, or even adequate descriptions of the implements. Readers have no way of knowing how the age estimate was made, and there is no assurance that the tools collected from within each site are even contemporary (as opposed to being the result of transitory occupations

over long periods of time), which examination of surface patina might disclose. Shackley implies that Mousterian tool-makers ("Neanderthals") were very recently living in this area, leaving their implements scattered on these open-air sites. Yet one cannot learn from her book what the implements looked like, whether they were classic Mousterian or a local variant, the basis of their age estimation, or whether a collection from a single site represents a single assemblage. Furthermore, under desert conditions, surface collections of stone implements might remain fresh and unworn although extremely ancient. 5) When local people are shown examples of Mousterian tools (identified by the author as "Neanderthal" tools), they aver that these tools had been used by people who once lived in the area, and who now dwell in the mountains and are called Almas (p. 163). If one could ask a 17th Century English farmer about the makers of stone implements found on his farm, he might well reply that the elves made them—they once lived here, but now live under the local long barrow. There is no *necessary* connection between folklore and reality.

6) Lastly, Shackley argues that the Almas are Neanderthals because of similarities between Neanderthal anatomy and behavior, and the reported anatomy and behavior of the Almas. Leaving aside the extent and pattern of body hair, about which no evidence exists for the Neanderthals, Shackley believes the Almas face has a small, flat nose and wide, Mongoloid cheekbones (pp. 114, 119), whereas the Neanderthal nose is extraordinarily prominent and the cheekbones are not pronounced. Figures 20 and 21 represent a drawing of an Almas reproduced from 1914, and Shackley's version of the same creature. The original 1914 description is not provided, but it is amazing that the same account could yield such disparate reconstructions. Shackley's version is much more hominid in appearance, and has lost its large canines, huge jaw, and sagittal crest. A small nose and wide cheekbones are still present in Shackley's figure, however, and, again, are not Neanderthal-like. Some reports of the Almas mention that they walk with their knees bent, their feet in-turned, and have a divergent hallux (pp. 104, 108). This is reminiscent of the classic bent-kneed gait and grasping big toe reconstructed for the Neanderthals by Boule in 1913, but this reconstruction of Neanderthal locomotion has been known to be false since the late 1950's. Discussing the Neanderthals, Shackley claims that skeletal remains often show evidence of rickets (p. 146), and that the Neanderthal hand was "rather primitive," with no precise manipulative abilities (p. 148). Both of these statements are incorrect.

There are also problems with behavioral similarities said to exist between Almas and the Neanderthals. The Almas are reported either to be incapable of speech, or else to speak in a very rudimentary fashion, supplementing vocalizations with gestures. Shackley claims that the Neanderthals also had "speech problems" (p. 159), and relied on gestures more than do normal,



modern humans. This claim is based on research which is highly controversial, but there also is the question of what Shackley means by "speech problems"—the original researchers only claimed that the encoding process of speech would be less efficient in Neanderthals than in modern human adults, and not that their speech would be severely affected. The Almas do not make or use fire, nor (with questionable exceptions) do they make or use tools. This behavior is obviously very different from archaeological evidence of Neanderthal behavior, but Shackley claims that "some degree of cultural recession is inevitable" (p. 161) if a hominid population has been driven into isolated regions. "Cultural recession" seems to have reached an extreme, because the Almas have no language, no control of fire, and no use of implements. Alternatively, anthropologists might argue that cultural elaboration is to be expected in response to severe environmental conditions.

Further strange observations appear in the last chapters. An Almas is seen butchering an ibex, and this suggests a link with the Neanderthals because they also hunted ibex (p. 161). Shackley herself had earlier termed this Almas report "dubious" (p. 105), but it now is used as evidence of Neanderthal survival. Is anyone who hunts caribou in the Canadian Arctic necessarily a surviving Caribou Eskimo? Old stories of an Almas captured in the 19th Century indicate that she had eventually learned to manipulate stones, smashing one stone against another; many years later, a "Neanderthal-style" tool is found on the hillside where the creature is reputed to have lived, and this is thought to be significant (p. 113). If the creature did exist, and did make this stone tool, does Shackley imply that there is some genetic basis for stone tool manufacture? This must be the case if the creature first had to be taught to manipulate stones, but later automatically produced a Mousterian implement.

The first chapter of this book concludes that wildmen and satyrs are mythological constructs. It is therefore ironical that the major thrust of the book—that Neanderthals are Almas and are still surviving—is based on a mythological Neanderthal reconstruction. The myth is that Neanderthals were shambling, stupid, speechless, bestial, and doomed in the competition with anatomically modern humans. This myth is not recognized by modern paleoanthropologists, although it flourishes in modern folklore.

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## Comments and Responses

*This section permits readers to critique or comment on works previously published in Cryptozoology. The original authors and other readers are encouraged to respond to these critiques or comments. Readers are also encouraged to critique or comment on the works appearing in this issue. All comments are the responsibility of the authors only, and do not reflect any policies established by the Editor or the Editorial Board of Cryptozoology, or the Board of Directors of the Society.*

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### A SOUND MIDDLE COURSE

(Comment on Krantz, *Cryptozoology*, Vol. 2:53–81)

This is the third time that I have crossed tracks with the Walla Walla Sasquatch footprints. My initial contact came about in November, 1982. Grover Krantz sent to me a set of 5 × 7 black and white photographs of the corresponding plaster casts, along with an invitation to analyze them, especially from the standpoint of dermatoglyphic features. I did not respond to that invitation.

My second encounter with the Sasquatch footprints was much more direct, and took place in August, 1983. I had organized a dermatoglyphics symposium in conjunction with the International Congress of Anthropological and Ethnological Sciences, that met that month in Vancouver, British Columbia. The symposium was well attended, and some two dozen dermatoglyphics specialists had come specially from Europe and the Middle East. Prof. Krantz approached me during the morning session to ask if he could show the Sasquatch casts to the assembled dermatoglyphists. This was arranged to take place during the lunch break, and I believe all of the specialists remained to take a look, although for varying lengths of time. There seemed to be a few who dismissed the casts outright, and departed, while another larger group (to which I belonged) examined the casts for a reasonable time with genuine interest. I recall some of the statements being made such as, "These sure do look like ridge and furrow systems," "How could anyone go to all the trouble of faking these," and, "If they are faked, the guy really knew his dermatoglyphics." Remarks of this kind hung in the air as I left. A few symposium participants did stay on, and, I presume, continued to make more detailed observations, and to discuss the nature of the casts with Prof. Krantz.

In this the third of my encounters, I will not attempt to assess the authenticity of the Sasquatch footprints as a form of heretofore unrecognized primate, nor will I seek to discredit the casts as part of a hoax. These are the only alternatives given by Prof. Krantz in his article, but I think there is a scientifically sound middle course, one which opts for no decision at this time for lack of substantive evidence.

What I would like to do is to evaluate certain arguments set forth by Prof. Krantz that are based on dermatoglyphic and anatomical features. The first matter is that of ridge breadth and ridge spacing seen on the plaster casts. Prof. Krantz indicated that the ridges were separated by about 0.5 mm, that this was typical for higher primates, and that during growth the spacing increases, so that males, being larger, tend to have more widely spaced epidermal ridges than females. It is my understanding that during growth it is the overall epidermal surface that increases in size, but differentially, so that ridge breadth expands more than the area between ridges. Thus, in the adult, ridges actually appear to be more closely approximated to each other. In addition to the growth factor, ridge breadth does vary in humans between the digits and palms, between hands, between sexes, as well as in certain chromosomal anomalies.

The Sasquatch footprints do show an interesting size relationship between the ridges and interridge distance. As near as I can determine from Fig. 13, the ridges appear to be roughly three times broader than the distance between them. In comparing this with casts that I have of Old World monkey palms, I found the Sasquatch ridges to be quite broad for their spacing. They tend to be densely packed, especially in an area where I would have expected wider interridge spacing. Of course, ridge compression during weight bearing could affect interridge separations.

The second matter concerns the co-occurrence of several rare conditions found on the Sasquatch footprints. In addition to the combination of anatomical peculiarities, consisting of a forward-positioned ankle joint, flat-footedness, and a very broad foot, there were reported digit malformations, including (possible) polydactyly, epidermal dysplasia (more precisely, ridge dissociation), and an absence of flexion creases. While it is plausible that certain of these conditions, such as polydactyly and ridge dissociation, could represent a single underlying medical or developmental syndrome, it seems most unlikely that all of these conditions would be manifested in a single individual. For the most part, the conditions do not seem to fit together very well, and until such time that additional evidence can shed light on their highly unusual co-occurrence, I think it would be premature to postulate a newly discovered primate.

In sum, I think the author should be commended for his thorough and dedicated investigation into the Sasquatch matter, and generally, he has offered cautious interpretations of the evidence. Where I differ from Prof.

Krantz is on his insistence at this time that a "clear conclusion" be reached, "as to either the method of hoaxing or to the reality of the animal." I rather think that, as in all areas of scientific "mysteries," time *may* provide conclusive answers, but only when sufficient evidence becomes available.

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## SIGNIFICANT IMPACT PREDICTED

(Comment on Heuvelmans, *Cryptozoology*, Vol. 3:1-30)

In the early 1950's, as a result of Willy Ley's excellent books, I became concerned with and interested in cryptozoology. Since that time, I have accumulated a great variety of cryptozoological information. However, none of these bits and pieces ever formed a coherent whole in my mind until I read Bernard Heuvelmans' superb article, "The Birth and Early History of Cryptozoology." One can only express admiration at the scope and completeness of the historical information presented. I also have to admit, with some chagrin, that not a few of the individuals Heuvelmans has cited were a complete surprise to me, in that I was unaware of their interest in, and contribution to, cryptozoology. The utility of the references cited alone makes this article invaluable.

But the historical synthesis of individual cryptozoological contributions, and the perspective provided by the article, represents only part of its value. I found most interesting the discussion of attitudes toward cryptozoology over time, which permits one to construct a frame of reference against which to judge current experiences. I found the balance of both popular and more scientific developments most gratifying, in that both the professional and the dilettante are given their due. Further, the international scope of cryptozoological events, as well as participants, are placed in a geographical and national perspective which significantly expands the overview. As a consequence of Heuvelmans' article, no present-day cryptozoologist need feel uneasy, in view of the eminent roster of distinguished scientists and scholars who have been interested in such zoological problems. How gratifying to discover that Sir William Hooker and Thomas Huxley took reports of "sea serpents" seriously.

A perennial question which is invariably asked by non-cryptozoologists is: "What major cryptozoological discoveries have been made of relatively large animals?" Without being tedious, Heuvelmans cites just three exam-



ples: the mountain tapir, the giant squid, and the okapi. Of course, others could be mentioned, including the recent transition of Tagua, the Pleistocene peccary, from cryptozoological to zoological status. It should be noted that, while it is true that for every cryptozoological success there may be ten or more failures or trivial conclusions, this rate of success is no worse than in the laboratory, where most scientists spend 90% or more of their time and effort doing the wrong things. It is that 5–10% of effort which pays off that makes it all worthwhile in cryptozoology as well as in more mainstream scientific research.

I must confess I began to read the section entitled "Discoveries which could have been made earlier with the help of cryptozoology," with some skepticism. However, I could not agree more with Heuvelmans' point that the indigenous people in an area are not generally fools or frauds, and should be listened to. I have always found in my own fieldwork that people who live in an area know more about the animals in the region, and their habits, than anyone else. When an old Eskimo woman living on the banks of the Skeena River in British Columbia, Canada, tells me about when and where the salmon migrate up the river, I listen carefully. I may not agree with her that the moon spirit is responsible, yet if she says the salmon run will peak on October 19, I take her information seriously.

Heuvelmans' article as a whole is the best "crash course" in cryptozoology I can imagine. I intend to have a stack of copies at the ready to give to interested parties and critics. I venture to predict that a significant impact will be made by this article on our effort to inform others as to what cryptozoology is all about.

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#### EASTERN COUGARS: THE VERIFIABILITY OF THE PRESENCE OF ISOLATED INDIVIDUALS VERSUS POPULATIONS

(Comment on Downing, *Cryptozoology*, Vol. 3:31–49)

Downing's considerable effort in the field in recent years has focused on finding evidence of cougar presence in likely wild areas of the Southeast, north of Florida. He writes: "Despite several years of intensive effort funded by the U.S. government, the author has been unable to positively confirm

that there are self-sustaining populations of cougars in the eastern United States, north of Florida." Downing's statement implies that a population of cougars may have gone undetected by his effort. We suggest that Downing's intensive, seemingly fruitless surveys for cougar sign may have substantial value in refuting the existence of cougar populations in the areas he censused, at the time he censused them. Our suggestion is based on the results of our own study in southern Utah, concerning the verifiability of cougar presence, using cougar track surveys.

The problem of verifying the presence of cougars in the East on the basis of sign (eyewitness sightings seem to be almost useless) hinges, in our opinion, on the different ranging behaviors of transient versus resident cougars. In his article, Downing concludes that "unless we accept the unlikely possibility that everyone who reported seeing a cougar in the East was mistaken, that animal has existed in small numbers in several areas at least within the last ten years." We concur with Downing, if he means that *wandering* individuals (including females with cubs) have existed and may presently exist in the East. Indeed, it is unlikely that such individual animals would not be present in wild areas, when one considers that escapees from menageries and zoos, as well as escaped pets, are not uncommon.

Even in New York, where the cougar is on the state list of endangered species (and where strict permit procedures would discourage ownership of pet cougars), there have been several documented instances of escapees that were subsequently recaptured or killed. The following is a quote by the endangered Species Unit Head, New York Department of Environmental Conservation: "Many puma sightings [in New York] . . . have actually proven to be accurate reports, but in all cases so far, the animal was found to be an escapee from a local menagerie or circus, or was simply released by someone who had been holding the animal or could no longer deal with it . . . . Only 6 months ago, a private small animal zoo tried to unload six cougars on the Department" (internal memorandum from Peter Nye to L. Meyers, 1977, on the "Van Etten Swamp Monster"). In his article, Downing lists several cases where escaped cougars were subsequently killed. In the State of Florida alone, there are currently about 1,000 registered captive cougars, according to a police department survey of permits (Chris R. Belden, personal communication).

It is likely that escaped cougars in the East that initially avoid recapture or death become wandering transients because they have no "social" reason to stay in any one area. On the basis of their intensive research on Idaho cougars, Seidensticher and associates postulated that transients will fail to colonize a new area unless there is an adjacent resident population of cougars (see John C. Seidensticher IV, Maurice G. Hornocker, Wilbur B. Wiles, and John P. Messie, 1973, Mountain Lion Social Organization in the Idaho

Primitive Area, *Wildlife Monographs*, No. 35). Areas in the West, such as Yellowstone Park, that used to support a cougar population prior to cougar extirpation by man, have tended to remain cougarless. For these reasons, we believe that proving the existence or absence of wandering transients may be statistically impossible because they do not leave a critical mass of verifiable sightings or sign. That is not to say that evidence of transients cannot occasionally be found. Chris R. Belden, a research biologist with the Florida Game and Freshwater Fish Game Commission, recently found the tracks of a single cougar in a wild northern Florida location (Chris R. Belden, personal communication). The tracks were found on only two occasions, spaced 6 months apart. Belden made monthly surveys.

On the other hand, we believe that the presence of a cougar *population* can be quantitatively verified with a reasonable amount of effort. In our study conducted with the Utah Cooperative Wildlife Unit, funded by the U.S. Fish and Wildlife Service and the World Wildlife Fund, we investigated track patterns and track densities produced by a population of transmitted cougars in southern Utah. We computed means and 95 percent confidence limits for the amount of effort (length of dirt road that must be searched) to locate the tracks of an individual telemetered cougar (see Fred G. VanDyke, Rainer H. Brocke, and Harley G. Shaw, Use of Road Track Counts as Indices of Mountain Lion Presence, *Journal of Wildlife Management*, in press).

The telemetry data showed that, excepting the passage of transients, cougars in this population had established home areas. We calculated the effort needed to locate tracks for various cougar population classes, including resident males, resident females, cubs, and transients, for four different road condition classes. For example, to be 95 percent confident of finding the tracks of a resident female, one would have to search 31.6 kilometers (19 miles) of dirt road ( $X = 21.3 + 10.3$  km) under good tracking conditions, or 78.5 kilometers (47.1 miles) on the average for all tracking conditions. These distances can be realistically covered in one day by a team of two surveyors in a slow-moving vehicle. By contrast, the equivalent value for transients was 188.4 kilometers (113 miles) of coverage needed ( $102.9 + 85.5$  kilometers). The latter data support our hypothesis that transients can remain undetected by ordinary survey efforts.

We propose to apply these data in systematic searches for cougars in the East. Preliminary information from telemetered cougars in Florida suggests that ranging patterns of cougars in that state are similar to those of the West (Chris R. Belden, personal communication). In the light of Downing's exhaustive surveys, our findings provide little hope that a cougar population exists in the East north of Florida. For cryptozoology in general, the greatest value of our findings may be that the existence of a population of cryptic

animals can be proved or disproved, given a specified level of survey effort, at an acceptable level of statistical confidence.

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## NO EXHAUSTIVE SURVEYS

(Response to Brocke and VanDyke)

I agree with Brocke and VanDyke that my work "... may have substantial value in refuting the existence of cougar populations in the areas he censused, at the time he censused them," but disagree with their later (last paragraph) description of my efforts as "exhaustive surveys."

I was only satisfied with the amount of effort expended in nine small areas, 20 to 50 km across, which I bisected three or more times in good tracking snow. Tracking in dirt in the mountains, where most people reported seeing cougars, was impractical. I doubt that all the mud puddles, fresh logging roads, and soft road shoulders I saw in 5 years of work in the Appalachians, laid end-to-end, would equal the 31.6 km indicated as needed by their study.

I wish them luck in their survey, and I hope they find useful our own model appearing elsewhere in this journal. I would merely caution them not to consider any area north of North Carolina to have been "exhaustively surveyed" by anyone.

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## YAHOO IN THE BAHAMAS

(Comment on Joyner, *Cryptozoology*, Vol. 3:55–57)

The explanation suggested by Joyner about the name Yahoo for the Australian hairy man sounds very likely, particularly since I know of at least one other instance where natives have adopted a “colonial word” for their own legendary “ape-man,” and, coincidentally, it is the same word Yahoo.

This other instance occurred in the Bahamas, where the natives of Andros Island, descendants of African slaves, spoke of “creatures like enormous hairy men, called by the Negroes ‘Yahoos,’ [which] are said to march about the woods in ‘schools,’ the largest coming first” (see John Gardiner, 1886, *Alligators in the Bahamas*, *Science*, Vol. 8:369). Considering the described behavior and the African origins of the Andros natives, the Bahamian Yahoos are probably derived from the memory of the African gorilla.

They have, in addition, two other “spirits” in their folklore, the *chick-charnies*, which, in my opinion, represent a memory of the African “little people,” about which Bernard Heuvelmans commented at greater length (see Bernard Heuvelmans, 1980, *Les Bêtes Humaines d’Afrique*, Plon, Paris), and the *lusca*, a tentacular sea monster said to inhabit the famous blue holes, probably a mythical version of the giant octopus.

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## THE YAHOO: A NATIVE ANIMAL WITHOUT A NATIVE NAME?

(Comment on Joyner, *Cryptozoology*, Vol. 3:55–57)

Joyner’s material makes a good case for a connection between the use of the Yahoo for the Australian hairy man and its use for the orang-utan. It must be doubted, however, that the connection is causal.

Citations in *The Oxford English Dictionary* make clear that, after its first use by Swift in his *Gulliver’s Travels* (1726) for an “imaginary race of brutes having the form of man,” the term was in popular use by the early 1800’s to signify a human being of degraded or bestial type. Thus, it was natural that, when non-literary substandard human forms appeared, they were dubbed *yahoo* in popular speech. This, Joyner points out, happened to “the orang-utan, the various ‘wild men’ of the 18th Century, and, at a later date, the gorilla,” as well as the Australian hairy man. All of this is a rather expected sequence of events. So the use of *yahoo* for the orang-utan—although it

undoubtedly reinforced the name in popular usage—is simply *parallel* to its use for the Australian hairy man, not causal.

While I agree with Joyner—perhaps from an ethnocentric point of view—that the usage of *yahoo* by the aborigines is probably due to their facility in assimilating English, I don’t think the possibility of a derivation from aboriginal sources can be discounted yet. A number of ex-aboriginal terms that suggest this possibility have worked their way into Australian English, at least at the slang or substandard level. Partridge, in *A Dictionary of Slang and Unconventional English* (1976), lists *yabber* “to talk, chatter”; *yabbie* “a crayfish,” and *yabby* “to fish for freshwater crayfish”; *yack* < *yakka* “food” (also *yacka*, *yacker*, and *yak*); *yakker* “food, talk, a contract, to work (at)” (Arthur Chipper’s *The Aussie Swearers Guide* [1972] gives *yak* “talk” and *yacker* “food,” as in *You’re all yak and no yacker*: “Your performance does not live up to your promises”); *yarra* “stupid, eccentric, crazy”; *yarra!* “stinking!”; *stone yarra* “completely mad”; *yarra-bankers* “loafers and down-and-outs”; *yarraman* “horse”; and *yan* “to go.” My own favorite is the Australian surfer’s *yabbadabba doo!* (c. 1961), which, if not ex-aboriginal, certainly shows native influence.

In short—if it is not too late—the aboriginal language should be examined more closely before it is discounted as the source of *yahoo*. Evidence of *yahoo*—or a term signifying the same creature—in the aboriginal language would serve to allay fears that the whole thing was not just a reffo (non-native) daydream.

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## CRYPTOWOMBAT OR CRYPTOHOMINID?

(Comment on Joyner, *Cryptozoology*, Vol. 3:55–57)

The reported Australian hairy man, or wild man, is a puzzle within an enigma, and for this reason I welcome Joyner introducing the subject in the pages of this journal. Australia is conspicuous by its absence in the works of the most authoritative pioneers of hominology, such as Heuvelmans, Sanderson, and Porshnev. True, the latter’s fundamental monograph (see Boris Porshnev, 1963, *The Present State of the Question of Relict Hominoids*, in Russian) had a few lines on Australia as a possible habitat of relict hominoids on the strength of ethnographic evidence (the aborigines’ folklore). But the idea seemed at the time so farfetched to Professor Porshnev’s colleagues and followers, including myself, that the possibility was never seriously discussed.

In the mid-1970's, with Boris Porshnev no longer alive, there appeared news media reports of the "Australian Snowman"—the "Yowie." My reaction was incredulity: it looked like Australian reporters were rehashing or simply copying the Yeti and Sasquatch stories and selling them as news about the new Yowie.

Then came Joyner's work (see Graham Joyner, 1977, *The Hairy Man of South Eastern Australia*, published by the author), and the pieces of the puzzle began to fall into place. It became clear that the Australian wild man, far from being an invention of present-day tabloids, has much evidence in its favor with the necessary signs of objectivity: a historical record of independent sighting reports from *both* aborigines and European settlers, plus claimed finds and fitting descriptions of appropriate footprints. These factors showed that mythology and sensationalism could not be the only explanations for the phenomenon. Hence my own recent paper (see Dmitri Bayanov, 1984, The Case for the Australian Hominoids, in Vladimir Markotic and Grover S. Krantz [eds.], *The Sasquatch and Other Unknown Hominoids*, Western Publishers, Calgary).

I am glad to learn from Joyner's new article in *Cryptozoology* that he also tends to accept the Yahoo-Yowie as a real animal. What I am not happy about is that Joyner makes it an animal quite different from mine. It turns out that his "hairy man" is not really man-like, and is supposed to be some kind of unknown wombat! To wit, "there is consequently no need to assume that the hairy man was man-like . . . Descriptions likening it to the wombat or native bear may be nearer to the truth . . . Perhaps the Yahoo was indeed some entirely unknown animal, like the projected Pleistocene species *Phascolarctos stirtoni* (Bartholomai 1968)."

Let us see whether this hypothesis stands up to the test of the historical material Joyner himself brought to light from the limbo of the archives. In his previous work (see Joyner, 1977, above), we find the following descriptions of the supposed wombat:

1) "The boy states that a wild man suddenly appeared from behind a tree, about 30 yards from the road, stood looking at him for a few seconds, and then turned and ran for the wooded hills a mile or so from the road . . . The boy states that he appeared to be 6 feet in height and heavily built. He described it 'as a big man covered with long hair'" (p. 9).

2) "Now it was in full view, and was of the stature of a man, moving with long strides and a heavy tramp . . . There were its footprints, long, like a man's, but with longer, spreading toes; there were its strides, also much longer than those of a man . . ." (p. 12).

3) "There were a good many blacks at the killing of it, and he saw two black fellows dragging it down the hill by its legs. It was like a black man, but covered all over with grey hair" (p. 13).

4) "... A strange animal . . . on all fours, was drinking from the creek . . . It rose to its full height, of about 7 feet, and looked quietly at the horseman. Then, stooping down, it finished its drink, and then, picking up a stick that lay by it, walked steadily up a slope . . . Summerell described the face as being like that of an ape or man, minus forehead and chin, with a great trunk all one size from shoulders to hips, and with arms that nearly reached to its ankles . . . Even in the prints which had sunk deepest into the mud there was no trace of the 'thumb' of the characteristic ape's 'foot'" (p. 16).

5) "They describe the douligah as a man of powerful build, capable of tearing down small trees and lifting great rocks. He had hair all over his body, and though he remained in the mountains during daylight, he frequently visited the abos' camps at night, and sent them scampering for shelter in caves" (p. 23).

6) "A doolagarl is a gorilla-like man. He has long spindly legs. He has a big chest, long arms. His forehead goes back from his eyebrows. His head goes into his shoulders, no neck" (24).

In Joyner's later work (see Graham Joyner, 1980, *More Historical Evidence for the Yahoo, Hairy Man, Wild Man or Australian "Gorilla,"* published by the author), we read an account by a shepherd and boundary rider, William Telfer, himself a purported Yahoo eyewitness, who relates what the aborigines told him about the subject (punctuation and spelling as in the original):

"They have a tradition about the yahoo they say he is a hairy man like a monkey plenty at one time not many now . . . at one time there were tribes of them and they were the original inhabitants of the Country before the present Race of aboriginals took possession of the Country he said they were the old Race of blacks . . ." (p. 1).

I hold there is not the least chance in the above for the wombat hypothesis, whereas the case for a pre-*sapiens* hominid seems to be fairly strong. Furthermore, the information on the Australian wild man is so similar, if not identical, with that on wild men in other parts of the world, that we have no choice but to treat all such information seriously, or reject all of it. I see no room for maneuvering, for geographically referential treatment here. And I cannot reject this Australian information until someone demonstrates convincingly how the similarity of such reports from far-flung world regions arises without a real animal being involved.

But, of course, in accepting wild men in Australia we run into a zoogeographical problem. A noted Soviet anthropologist has rejected the Patterson-Gimlin Sasquatch film out of hand "because there are no apes in America." Fortunately, we do not need apes in America to prove the authenticity of the film. But how can we defend wild men in Australia, which is known to have originally been populated by only two species of major placental mammals, *Homo sapiens* and his dingo dog? If other large placental animals did



not make it to Australia, how can we claim that wild men could? My answer is: because, in many respects, wild men are not on a par with other animals. If we accept that they are, or can be, hominids, then by definition they are the closest relatives of *Homo sapiens*, and share as yet an unknown number of common characteristics like him. Since *Homo sapiens* has made it to Australia, how can we rule out the possibility that other hominids did not do the same?

As other Asian animals did not simultaneously reach Australia, we must conclude that there remained substantial water barriers between Asia and Australia at the time. How could wild men have crossed such barriers? Apparently, by swimming. *Homo sapiens* is known to cross the water-gap between France and England by swimming. And what information do we have on the aquatic ability of the wild man? Bernheimer cites the earliest European data on this score: "... Aquatic wild men are not always limited to a habitat in stagnant ponds, and one, described as tailless and hairy but bald, is supposed to have been caught in 1161 in the sea near Orford on the English coast, and to have been dumped back again, when it turned out that nobody could make him talk" (see Richard Bernheimer, 1952, *Wild Men in the Middle Ages: A Study in Art, Sentiment, and Demonology*, Harvard University Press, Cambridge).

As for the North American hominoids, John Green, in his magnum opus, devotes a whole chapter to "Apes Under Water" (see John Green, 1978, *The Apes Among Us*, Hancock House, Saanichton, British Columbia). The evidence he cites closely corresponds to the information obtained in the Soviet Union on the aquatic ability and proclivity of relict hominoids, who are said to easily swim across very wide Siberian rivers.

So, could Australia have initially been colonized by swimming hominids? That is the question.

I find the problem of the Australian wild man one of the most fascinating and challenging in hominology. Its elucidation may shed light on the nature and identity of relict hominoids in general, and even on the question of *Homo sapiens*' penetration into Australia. I therefore invite specialists of different disciplines to continue discussion of the subject in *Cryptozoology*. Opinions of Australian scholars should be especially welcome. For lack of space, I have not touched on the provenance of the name Yahoo as applied to the Australian hairy man, but I think that the question of name is of peripheral importance to the question of the creature's reality and identity.

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## SCIENCE AND THE AUSTRALIAN WILD MAN

(Response to Raynal, Becker, and Bayanov)

The information provided by Raynal is interesting. Tony Healy, an Australian who visited the Bahamas a few years ago, told me that he had heard the *yayhoo* spoken of there. The point about the use of Yahoo in England and Australia in the early 19th Century is that there was a definite migration of persons from the one to the other at that time, which could account for the transmission of the word. I do not know whether the same could be said for Africa and Andros Island.

Becker believes that both the British use of Yahoo for a large ape (presumably the orang-utan) in the early 19th Century, and the somewhat later application of the word to an Australian wild man, arose independently from the use of the word to describe a coarse or degraded human being. Becker theorizes that a word can flow out into a language naturally or even logically from a single source, without any interruptions or awkward cross currents. However, in applying this theory to the present case, he has to ignore evidence to the contrary. For instance, the gorilla was *not* called Yahoo as, according to Becker's theory, it should have been. As for the Australian aborigines, they did indeed have words beginning with the sound *ya*, but why would they have used precisely the same word that Swift had invented?

I thank Dmitri Bayanov for his comments, particularly for his contribution concerning the zoogeographical problem involved, but he has perhaps missed the meaning I intended in the appendix to my argument. I wrote: "There is consequently no need to assume that the hairy man was man-like." Of course, one can, and many do, assume it, but it would be wrong to ignore other possibilities. I should also draw attention to the words "may" and "perhaps" in the last paragraph.

I agree with Bayanov that it would be useful to continue discussion of the supposed Australian wild man in *Cryptozoology*. To that end, I shall add a few remarks of my own about method.

Zoology generally requires the production of material evidence to support the formal identification of species. Empirical science, however, is actually not concerned with the attainment of certainty, but, rather, with the application of rational thought to perception of the phenomenal world. Where the only available evidence takes the form of records of sightings over a long period of time of some unknown creature (i.e., sporadic transient phenomena), then the correct method to use is that called "historical." It is important to realize that statements of this kind about past events are (or purport to be) records of empirical observations, and there is nothing unscientific about making use of them, provided that they are subjected to critical analysis, including some suitable method of verification.

The best single method of verification for this kind of material is a correlative one. This requires, of course, that the observations be independent of one another. Independence can usually be assumed where there is temporal or geographical separation, especially in circumstances where communication is poor. Even where observations are contiguous, independence may sometimes be assumed where the later account introduces some information not recorded in the earlier one. There are also other factors which may help to establish the truth or falsity of an account, such as the character of the observer, the intention of the writer, or the internal consistency of the text.

In conclusion, one can say that, where the required conditions are absent, there is a need to proceed with extreme caution. For instance, if accounts of sightings lack independence by being kept constantly before the public, or if their proximate source is the same, while their ultimate source is not clearly identified, then it is hard to make anything much of them.

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#### NEW MACAQUE HYPOTHESIS SUPPORTED

(Comment on Zhou, *Cryptozoology*, Vol. 3:58–70)

Zhou's article mentions the possible presence of a new species or subspecies of stump-tailed macaque. Zhou suggests the possibility that this potentially rare animal may account for reports of a shorter Wildman in the Shennongjia region of Hubei Province, People's Republic of China. My previous experience in Hubei Province may help strengthen Zhou's suggestion.

In 1982, we conducted primatological research in Xhin Xhan County, Hubei Province, an area immediately adjacent to Shennongjia. As we previously reported (see Frank E. Poirier, Hu Hongxing, and Chung-Min Chen, 1983, The Evidence for Wildman in Hubei Province, People's Republic of China, *Cryptozoology*, Vol. 2:25–39), there are reports of Wildman in this region. We suggested that some of these reports were actually of the very rare and endangered snub-nosed golden monkey, *Rhinopithecus roxellanae*. Zhou's report, however, raises another possibility, one that we did not seriously consider at the time. Throughout our stay in Xhin Xhan County, we heard reports of another macaque species inhabiting the area. This animal was said to be much larger than the macaques normally resident in the Xhin Xhan region, was reported to have no tail or a short stubby one, a larger face, and a darker hair coloration. The macaque was often referred to as the

"Bear Monkey." Being unable to actually view the animal, we rejected the claim, and suggested instead that the indigenous people were merely citing intraspecific morphological variability.

Zhou's report of the possibility of a new, larger macaque species or subspecies in Shennongjia suggests that we should have placed more credence on local reports. Could it be that some of the reports of a smaller version of the Wildman in the Xhin Xhan region are mistaken identity for a new macaque species, as Zhou suggests? As an interesting sidenote, we ourselves never heard reports of a supposed smaller version of the Wildman.

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#### NEW MACAQUE HYPOTHESIS NOT SUPPORTED

(Comment on Zhou, *Cryptozoology*, Vol. 3:58–70)

In this eagerly awaited report, Zhou provides us with the most detailed description to date of two widely publicized events of possible cryptozoological import, the recovery from Zhejiang Province of a set of hands and feet belonging to an alleged Wildman, and the capture of a large monkey of uncertain affinities from Anhui Province. Zhou finds many similarities between these two individuals, and suggests that they both belong to an undescribed species or subspecies of stump-tailed macaque.

Based on the information presented in the article, however, I feel that there is no need to postulate a new taxon, and that the two primates in question can be allocated with reasonable confidence to a known species of stump-tailed macaque, *Macaca thibetana*. This large and exceedingly rare monkey is one of two species of stump-tailed macaque known to inhabit China, the other one being *M. arctoides* (= *M. speciosa*) (see Jack Fooden, Quan Guoqiang, Wang Zongren, and Wang Yingxiang, 1985, The Stumptail Macaques of China, *American Journal of Primatology*, Vol. 8[1]:11–30.) In his paper, Zhou considers *M. thibetana* to be a subspecies of *M. speciosa*, while Fooden and his three Chinese co-authors regard them as very distinct species.

The recently captured Huang Mountain monkey is from an area known to yield specimens of *M. thibetana*. The photograph Zhou provides shows that the Huang Mountain monkey has the following features characteristic of *M. thibetana*: prominent beard and cheek tufts, forehead and brow ridges



thickly furred, and brown pelage on the back that gradually pales toward the ventral surface of the body. Zhou states that the monkey (a male) weighs in excess of 20 kg. The maximum known body weight for male *M. thibetana* is 17.5 kg ( $n = 5$ ,  $\bar{x} = 15.2$  kg,  $OR = 14.2$ – $17.5$  kg). However, since Fooden and his associates were working with a sample of only 5 individuals, this discrepancy is hardly noteworthy.

Zhou observes that the nails of the Huang Mountain monkey (and those of the Juilong Mountain specimens) are notably flat, and he stresses this feature in separating these creatures from known macaque species. The flatness of nails strikes me as a very poor criterion for species diagnosis, especially when there seems to be little else that favors the introduction of a new taxon. Zhou's fingernail and toenail shape indices (Tables 1 and 2) do not clearly discriminate the Juilong and Huang Mountain monkeys from known macaque species, and while these indices do not exactly quantify nail flatness, the data do show that individual nail proportions are highly variable.

The detached cheiridea of the Juilong Mountain specimens can be most parsimoniously assigned to *M. thibetana* on the basis of their large size, close resemblance to the cheiridea of the Huang Mountain monkey, and their geographic location, which falls within the distributional boundaries of *M. thibetana*.

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## SOMETHING MORE

(Comment on Janis, *Cryptozoology*, Vol. 3:141–44)

I was very interested in the points raised by Janis in her excellent Comment on Agnagna. She ended by stating that she did not think she could “say anything more,” and “nor, for that matter, could anybody else.” I would now like to attempt to say something more, lest this challenge go unmet. (As some readers may know, I participated in the 1981 Mackal Expedition to the Congo, and have given a great deal of thought to the question of Mokele-Mbembe, including its possible sauropod affinities.)

First, although I know Marcellin Agnagna well, having spent some time with him in Paris and Rome, as well as being with him during “hazardous duty” in the swamps, I will not try to defend him. He is a trained, professional

zoologist, and his Field Report (see Marcellin Agnagna, 1983, Results of the First Congolese Mokele-Mbembe Expedition, *Cryptozoology*, Vol. 2:103–12), including his personal observation of Mokele-Mbembe itself, stands on its own strength without defense from others.

Instead, I will address other points which *can* be objectively defended. One comment to Janis by one of her colleagues concerned the destruction of habitat by African elephants, and that “a dinosaur would be five times that size; you couldn't sustain a small local population of animals that big.” Janis' response to this problem was to address the question of endothermy (“warm-bloodedness”) versus ectothermy (“cold-bloodedness”), pointing out that ectothermic sauropods would consume much less vegetation, and thus leave much less evidence of their passage. This is an important point, because, as already pointed out in this journal, an (endothermic) elephant weighing 3,672 kg needs to consume about 49,000 kcal per day (see Charles W. Weber, James W. Berry, and J. Richard Greenwell, 1982, Mokele-Mbembe: Proximate Analysis of its Supposed Food Source, *Cryptozoology*, Vol. 1:49–53), and a herd of bush elephants spending some time on an East African savannah would thus very soon leave localized areas degraded—as indeed they do.

However, a number of qualifications need to be made at this point. First, Mokele-Mbembe, if it is a sauropod, would be a small one, *not* “five times” the size of an elephant. On the contrary, its size (excluding its reportedly long, thin neck) is said to be intermediate between that of a hippo and an elephant. In this case, the elephant we are dealing with—the only one known to the natives of the Congo Basin—is the forest elephant subspecies, *Loxodonta africana cyclotis*. Although some specialists do not recognize this animal as an actual subspecies, considering it only a “forest runt,” the fact remains that it is much smaller than the more well-known bush elephant of East and Southern Africa. These facts, apparently unknown to Janis' colleague, reduce Mokele-Mbembe's size to more modest proportions.

Second, although elephants move in herds, and although many paleobiologists now believe sauropods also moved in herds, the Likouala natives report Mokele-Mbembe moves singly or in pairs. Thus, any habitat destruction would be randomly dispersed and far less visible.

The third important point is that the habitat of Mokele-Mbembe is *not* the relatively pleasant East African savannah, which most people visualize when thinking of Africa, and which can be accessed easily by Land Rover for hunting or tea parties. We are dealing with a thickly vegetated swamp-forest, of about 50,000 square miles, which can only be penetrated in parts, and with great difficulty, by dugout canoe or on foot. Evidence for a herd of forest elephant-size herbivores (even endothermic ones) would not be noticeable in such an area, first because the vegetation destruction would be utterly lost in the enormity of the forest (something that can only be truly



appreciated by those who have been in "big" rain forest as is found in the Amazon or Congo Basins), and second, because there would be nobody there to see it in the first place. The Likouala has a population of less than one person per square kilometer, most of it concentrated in small villages on the rivers, *not* in the forest, and all are native people—the very ones who occasionally report Mokele-Mbembe. To my knowledge, no actual zoologists had ever entered the Likouala prior to the recent expeditions, so there is little point in talking about the evidence which would be visible from habitat destruction.

A final point related to all of this is that, even if such enormous habitat destruction occurred due to unlikely herd's of roaming Mokele-Mbembe's, the evidence would very soon be obliterated by rapid forest growth, again, something which does not occur in the more arid savannahs of Kenya or other East African countries, which, it seems, Janis' colleague is more familiar with.

All of these points are quite separate from Janis' own observations on the endothermy/ectothermy question, and they solve the problem without even having to resort to that question. The verification of this is quite simple: four expeditions have entered the Likouala in the past 5 years, and none have observed *any* habitat destruction by elephants, despite the fact that elephants are there.

When one is in the Likouala swamp-forest weighing all of these factors—as, indeed, we did—and observing the enormity and impenetrability of the region, one realizes that there could be hundreds, even thousands of dinosaurs in there, without Western science having the slightest knowledge of it. That does not mean they *are* there, only that it is possible.

And that, I think, is just about all I or anybody else can say about it.

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## INSTRUCTIONS TO CONTRIBUTORS

### General

All manuscripts submitted for publication in *Cryptozoology* must be typed double-spaced (if possible on 8½ × 11 inch or 22 cm × 28 cm sheets). American rather than British spelling and style are preferred (e.g., periods and commas placed *inside* of quotation marks). The author's last name must appear in the top, right-hand corner of every manuscript page.

### Articles

Manuscripts must be submitted in triplicate, and must be prefaced by 50–100 word informative abstracts. The abstract should appear on the bottom half of the title page. The manuscript should begin on the second page. The citation of references is not mandatory, but is preferred. If references are cited, the author(s) must conform to the referencing instructions appearing below. Manuscripts should contain relevant descriptions or interpretations related to cryptozoological matters. Topical or theoretical discussions or literature reviews, rather than specific research findings, are preferred. Illustrations and maps are welcome. Original artwork and black and white photographs are required (color slides are not acceptable). The author(s) should design subheadings in accordance with the structure of the article. Manuscript length is left to the discretion of the author(s), although a minimum of 1,000 words is expected. Manuscripts will be reviewed by two referees for scientific content, originality, and clarity of expression, and the Editor may suggest deletions or revisions.

### Research Reports

Manuscripts must be prefaced by 50–100 word informative abstracts. The abstract should appear on the bottom half of the title page. The manuscript should begin on the second page. Only one manuscript copy need be submitted. The citation of references is not mandatory, but is preferred. If references are cited, the author(s) must conform to the referencing instructions appearing below. Manuscripts should detail specific research findings which can be quantified. Photographs, tables, and graphs are welcome. Examples would be results of laboratory analyses, photographic analyses, or social science or survey analyses. Subheadings should be: *Introduction* (which should give some background on the problem in general and the case in question in particular), *Method, Results, and Discussion*. Manuscript length is left to the discretion of the author(s). A minimum of 1,000 words is expected.

### Field Reports

These manuscripts should report on cryptozoological fieldwork. They should be limited to a maximum of 2,000 words (longer manuscripts will be considered in special circumstances). Only one manuscript copy need be submitted. Formal referencing should *not* be prepared, although references may be cited (in parentheses) *within* the text when appropriate (see examples in the journal). Subheadings should be: *Introduction* (in which the type of cryptozoological event investigated is detailed, and the purpose or aims of the fieldwork, and the location and dates involved, are given; previous fieldwork by the author[s] in the same geographic region should also be mentioned); *Narrative Description* (in which the field activities are described, e.g., lake surveillance, equipment, interviews with aboriginal peoples); *Results* (in which the outcome of these efforts is described; the total lack of success of such fieldwork will not be used as a criterion in evaluating manuscripts); *Future Plans* (in which a brief discussion or statement on whether the author[s] plan[s] further fieldwork in the area is provided). Black and white photographs and maps are welcome.

### Book Reviews

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